THE FUTURE OF MEDICINE

SIR JAMES MACKENZIE

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BY

SIR JAMES MACKENZIE,

F.R.S., M.D., F.R.C.P., LL.D.Ab. & Ed., F.R.C.P.L. (Hon.)

Consulting Physician to the London Hospital.

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TO THE

RIGHT HONOURABLE VISCOUNT KNUTSFORD

A zealous friend of Medical Science.

WHOSE INTEREST IN MEDICINE IS NOT ONLY PERSONAL BUT HEREDITARY, AND WHOSE DEVOTION TO THE RELIEF OF SUFFERING HAS LED HIM TO TAKE AN ACTIVE AND SYMPATHETIC PART IN THE PROGRESS OF MEDICINE.

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PREFACE

MEDICINE is a subject slowly evolving out of a past in which facts and fancies, faiths and beliefs, and even superstitions, were strangely commingled. During the past few centuries it has been gradually shedding many of these beliefs and is daily becoming more exact in its methods, and basing its practice more on reason and less on faith. The subject however is so vast in extent, and its aspects so numerous and varied, that it is difficult to comprehend all its bearings. Advance in knowledge is taking place on such a wide front, that the question may arise whether the concentration of the forces upon one part of the field which, for the time being, is yielding some fruitful results, may divert the movement into paths that are not best suited to achieve the great purpose of medicine. It behoves us to pause, from time to time, to consider what we are doing and whither we are tending. Such a review I have undertaken and the conviction has been forced upon me that the conception of medicine dominant to-day does not direct the pursuit of it by methods best adapted to attain its chief aim.

In order that the reader may understand the conception of medicine, which is the outcome of this review, I give in the first Chapter an outline of the argument, which will be developed with some fulness in the rest of the book. After indicating what is the

chief purpose of medicine, and showing where the methods pursued to-day fail to attain that purpose, the argument proceeds to describe the limitations in education, in practice and in research. From a description of personal experiences, certain inferences can be drawn, which form a basis on which a constructive policy can be developed, and which it is hoped will place the prosecution of the knowledge of medicine on lines helpful to a better knowledge, till it is superseded by another conception. I recognise quite clearly that dealing with such a vast subject, and possessing a dim perception of only a portion of the field, it is vain to suppose one can tell what the morrow may bring forth.

J. M.

New Park,
St. Andrews, N.B.

July, 1919.

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PART I.

CRITICAL

INTRODUCTION

To be successful in any endeavour it is necessary to have a clear idea of the object towards which we strive. The chief aim towards which all endeavours should be bent, in medicine, is the prevention and cure of disease. While that should be the aim, its achievement is difficult, for the greatest efforts of the profession are diverted to the more urgent call—the relief of suffering, and the improvement of impaired health. Disease is rarely recognised until it has impaired the health of the individual, and produced suffering; and the concentration of attention to this stage has diverted attention from the preceding stages.

The Four Stages of Disease.

While recognising that disease progresses by a gradual development, it might be well to picture it as consisting of four stages.

First.—The Predisposing Stage—that is the stage in which the individual is free from disease but liable to be attacked either from some inherent weakness or from an outside source.

Second.—The Early Stage—when the disease has entered the human system but has not produced any

perceptible alteration of tissue, when the signs the disease produces are mainly subjective—this is also the curable stage.

Third.—The Advanced Stage—when the disease has progressed so far that it has caused destruction or modification of tissue, and when its presence is revealed by a physical sign.

Fourth.—The Final Stage—when the individual has died, and when the tissues are subjected to a post mortem examination.

Most subjects can best be studied by starting at the beginning and continuing to the end. The gradual evolution of medicine has shown that disease can best be studied by starting at the end. Until pathology obtained for itself a prominent place among its departments, the practice of medicine was largely speculative. The gradual development of pathology threw so much light upon disease that it helped greatly to raise medicine towards being a science. The importance of pathology is now so universally recognised, that everywhere facilities are given for its prosecution, so that it can be said, that ample provision has been made for the study of the disease, after it has killed its victim.

Clinicians were the first to appreciate the importance of pathology, and speedily applied the knowledge acquired from the post mortem study of disease, to the interpretation of clinical phenomena. By co-relating the signs evoked during life, with the state found post mortem, the basis was laid for a sound diagnosis of the physical signs of disease. The clinician also benefited by the great extension of surgical procedures, in that he was able to utilise their results in the same way. The progress, made in the different laboratory departments, also gave new methods which the clinician has adopted. Thus there has been a vast accumulation of knowledge pertaining to the third stage of disease.

It must be recognised that this development is, to all intents and purposes, restricted to the recognition of disease after it has advanced so far as to have damaged the tissues, a stage which, except in rare instances, does not permit of our attaining one of the chief aims of medicine—cure. Indeed, so great has been the development of means to recognise and treat this stage in the past, and so dominant have the methods become to-day, that the chief aim in medicine has been, to a large extent, obscured.

The Early Stages of Disease.

Medicine has advanced so far, that for the study of disease after the patient has died, we find institutions magnificently equipped, presided over by men of great experience and training; for patients suffering from the advanced stages of disease, we have great hospitals, with staffs of skilled physicians, surgeons and specialists. If we seek to find out, "What are the facilities offered for the detection and cure of disease in the stage when it has not damaged the tissues?" we discover that there is little consideration given to this aspect of the matter. It is indeed instructive to reflect, that, while men undergo a long and special training to enable them to recognise the appearance of disease after the patient has died, and other men undergo equally careful training to enable them to recognise disease after it has damaged the tissues, few or no attempts are made to train men for the detection of the disease when there is a hope of cure. There is, it is true, an impression that the methods of diagnosis are so developed, that disease is now recognised at the earliest stage at which it is humanly possible to recognise it, and that any phenomena that occur earlier, are so vague and indefinite, that no clear information can be obtained from them. This view is not justified. There

are evidences which would surely indicate the nature of the disease in its earliest stages, were we capable of detecting them; the reason why we fail to detect these

The proportion of diseases in 1,000 consecutive cases seen in the consulting room representing the slighter ailments that impair the health of the community, some of which probably predispose to the diseases of which the people die.

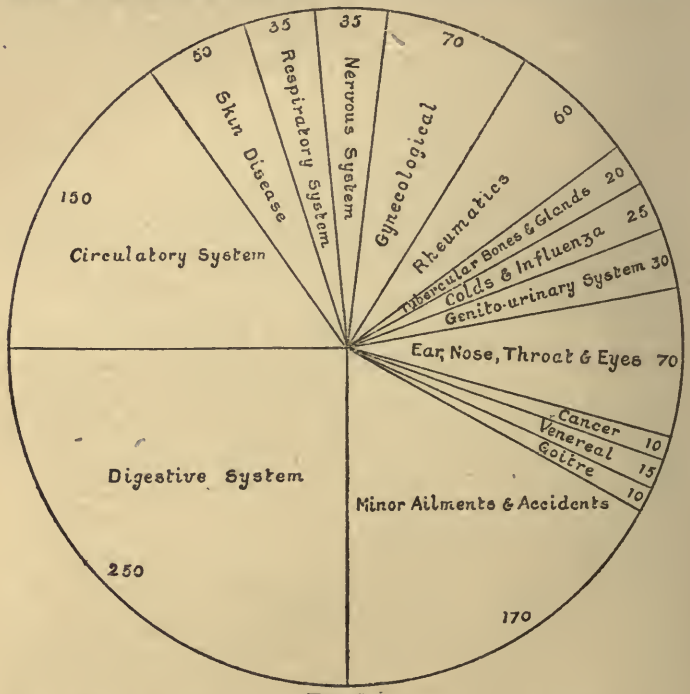


FIG. 1.

The object of these diagrams (Figs. 1 and 2) is to show (1) The difference between the diseases that impair the health, and those from which people die; (2) To support the suggestion that the diseases that impair the health may predispose the individual to the diseases which kill; (3) To bring clearly out how little advance has been made in preventing and curing the more common diseases.

evidences is, that medical knowledge has not yet progressed far enough to inform us how to set about finding them.

The Predisposing Stage of Disease.

If we do not know the early signs of disease, there is little hope of our achieving the aim of medicine—the prevention of disease. The study of the early stages of a malady brings us near to the circumstances which provoked that malady. When the recognition

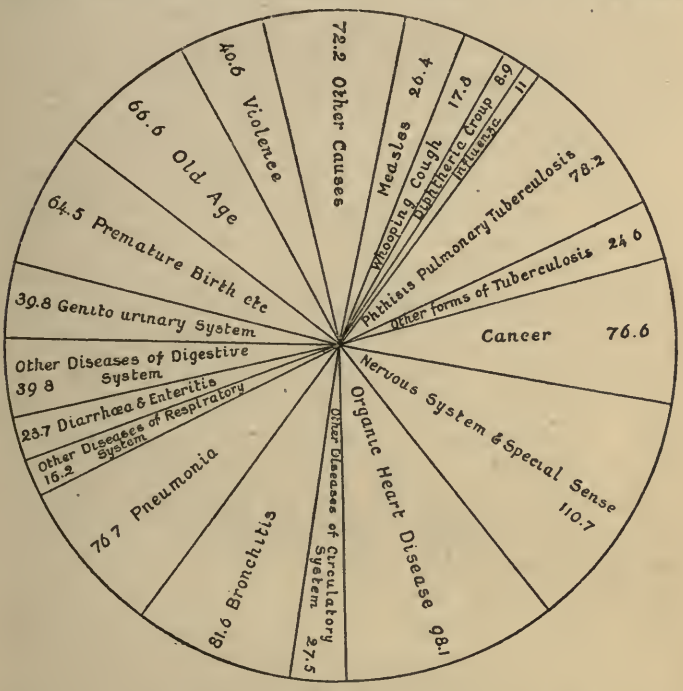


FIG. 2.

ENGLAND AND WALES.—PROPORTION OF DEATHS FROM THE PRINCIPAL CAUSES TO TOTAL DEATHS. 1912. (*Sir George Newman.*)

of disease takes place a long time after its inception, the circumstances provoking it cannot be appreciated; hence that part of medical knowledge, included under the term etiology, is generally little better than guess work. The need for the prevention of disease has received wide recognition, and the laws of public

health, and the application of the principles of hygiene, have done a great deal in stamping out certain diseases, and diminishing the frequency of others, while the recognition of the microbic origin of many diseases, has also given the indication for their suppression. But when we consider the great number of diseases that afflict the community, it will be seen that our ability to prevent disease is extremely limited, and that the advance made merely touches the fringe of the subject. In Figures 1 and 2 are two diagrams, the one indicating the diseases that impair the health of the community, and the other the diseases of which the great bulk of the community die. It is not contended that either of these diagrams give an actual representation of the diseases, but they are sufficiently accurate to enable us to realise how slight is the effect of preventive medicine upon the more common ailments of a community. The first diagram represents the kind of complaints from which the people suffer in the earlier stages, when they are able to be up and about and can come to the doctor's consulting room. It is constructed from the records of one thousand consecutive cases. Probably another thousand would show some differences, and another observer give a different diagnosis in many cases. Still it represents the matter with sufficient accuracy to enable certain deductions to be drawn. The second diagram is given by Sir George Newman to show the diseases of which the bulk of the population die. Here again the table is not to be considered an accurate representation, for the diagnoses are not always strictly accurate, and often give only the disease whose symptoms were most in evidence. The contrast between the two is striking, and, unreliable as they are, it can be inferred that the diseases that weaken the community are not those of which the people die. The suggestion arises, that there

are many simple diseases which render the individual susceptible to the more serious diseases. Thus in the first diagram, twenty-five per cent. of the cases suffered from some trouble of the digestive system, while in the second diagram, only a little over six per cent. died of diseases of this system. The suggestion arises that diseases of the digestive system, by weakening the body, predispose to other diseases. That there is some truth in this I shall endeavour to show later.

The main reason I have for giving these diagrams, is to show how little effect the progress of medicine has had in preventing the occurrence of the most common diseases, and to bring out clearly the need for reconsidering the attitude of medicine towards such diseases.

The Problem and steps necessary for its Solution.

Medicine, having advanced so far as to have revealed the fourth and third stages of disease, is now confronted with the problem of understanding the first and second stages. Obviously the methods, sufficient for the investigation of the last two stages, are not suited for the investigation of the first two. The next step is to recognise, that if progress cannot be carried further with our present conceptions and methods, we must look out for a new concept, as well as new methods. These may not be discoverable from the mere contemplation of the subject, but the problem being recognised, a persistent endeavour to discover the defects in our methods may result in a better understanding of the difficulties, and a realisation of the methods by which they can be overcome.

It was difficult to know at first how to proceed with such a subject, but as one advanced and acquired a fuller knowledge of disease, certain principles began to emerge which served as a guide.

The first of these is that *if a problem is to be solved*

the nature of the problem and its difficulties should first be comprehended.

Applying this to the problem of medicine, we should have a thorough knowledge of disease in all its bearings —the conditions favouring its onset, the symptoms it provokes, and the course it runs. On a review of the more common diseases, as shown in Figs. 1 and 2, it becomes doubtful if we possess a sufficient knowledge of one of them.

A second guiding principle arose from the consideration that, *as disease is only made manifest to us by the symptoms it produces, to get a thorough understanding of disease, we must recognise the symptoms not merely by detecting their presence, but by understanding the mechanism of their production, and their bearing on the future health of the patient.*

It is supposed, or taken for granted, that this has already been done, for it has been the object of clinical investigation for untold years, and the recent developments of medicine have been largely concerned with bringing to light new manifestations. But here again, the manner in which the investigation of symptoms should be pursued, has not been understood, and it may be taken for granted that the progress of medicine will be greatly hampered until the study of symptoms has been thoroughly prosecuted, on lines different from those that have been followed in the past.

From this consideration another principle appeared, *that the individuals, conducting an investigation, should have had the opportunity of seeing disease in all its phases, and of observing the symptoms it produces.* This principle is so self-evident that it seems a waste of time to consider it, yet it has been systematically neglected in the investigation of disease. If the conditions which predispose to, or provoke disease, are

to be recognised, the investigator must have the opportunity for seeing the circumstances which led up to the invasion of the disease. It is manifest that neither the laboratory worker nor the hospital physician, who are the persons mainly concerned with research, have this opportunity. The early stages of diseases are, as a rule, insidious, and are indicated mainly by subjective sensations. The patient becoming conscious that there is something amiss with him, does not, as a rule, seek help from the hospital physician, but from his family doctor. The bulk of patients, in the early stage of disease, are never seen by those who are systematically engaged in its investigation.

Complete understanding of the symptoms provoked by disease is not limited to the detection of the symptom and of its mechanism, but necessitates a knowledge of the bearing of its cause upon the patient's future; in other words, the investigator must be able to assess the value of the symptoms. To obtain this knowledge he must have opportunities of watching individuals for long periods—opportunities which are denied to the hospital physician and laboratory worker.

The General Practitioner and Medical Investigation.

If then, to achieve the aim of medicine, it is necessary to recognise disease, and understand all the phases of its life history, it is evident that only one class of individual has the opportunity for acquiring this knowledge, and he is the general practitioner. His opportunities, as the family physician, enable him to become acquainted with the lives of a number of people, through seeing them before disease attacks them, and watching them during the whole course of its development. He has also the opportunity of observing all kinds of diseases, and the inter-play of primary and super-added diseases. He is the only individual who

has the opportunity for finding out the significance of the various signs, a knowledge which is absolutely essential to the investigation of disease, as well as to the rational practice of medicine. His opportunities give him a far wider outlook upon disease than any other members of the profession, however experienced in special branches. The need for someone with a broad outlook is imperative, as the modern tendency towards specialisation restricts all investigators and teachers to a narrow sphere of experience.

It might be said, that the general practitioner has had, from all time, the opportunity for investigating disease in the manner I am advocating, and that he has failed to utilise this for the advance of medicine. In one sense that is true, though it should be remembered, that up till recent years, the teachers of medicine, and those who have done so much to advance our knowledge, have been, to all intents, general practitioners, such as Harvey, Hunter, Jenner, and many others. With the development of laboratory methods, and the influence they have had on medicine, the notion has arisen that progress can only be made by their use. Therefore, all facilities have been given to those who could use these methods; indeed so little was it realised that the general practitioner only had opportunities for exploring fields essential to the progress of medicine, that his education was never directed towards equipping him for such work, while it was impressed upon him, that for research, special methods and facilities were necessary from which he was excluded. If he ever should have any desire to prosecute research, it will be seen that the prevalent attitude towards medicine tends to blind him to his opportunities; moreover his education has not been such as would render him capable of utilising these opportunities.

The Complexity of Medicine.

It will probably be admitted, that while the general practitioner has all these opportunities, other workers can obtain the kind of knowledge I refer to by other means. In the out-patient departments of hospitals, the early stages can be seen; while the examination of a large number of people, at different stages of a disease, gives the information necessary to an understanding of the progress of disease, and the significance of symptoms. In the development of the argument I shall show that these methods fail and why they fail.

Another objection to the general practitioner taking part in research and in teaching, is that medicine has become so complex, that his knowledge of any particular disease is bound to be far less than that of men who devote their whole energies to certain limited fields. Moreover, the methods employed for the detection of disease have become so numerous, and so recondite, that one individual can only be adept with a few, i.e. those concerned with the speciality he professes. In other words, medicine has become such a complicated subject, that it is hopeless to expect any advance to be made, except by those who are familiar with the methods of special branches. Here again lies a fallacy. The fact that medicine is becoming so complex implies that it is being pursued on wrong lines, for a subject which is based on natural laws, becomes easier to understand as the laws become better known. That medicine can be simplified, and rendered easy of comprehension, is one of the points which I will endeavour to demonstrate.

To sum up, the progress of medicine is being hampered by an imperfect conception of the aims of medicine, and a consequent employment of methods that fail to advance the subject. So far, the greatest endeavours have been spent in elucidating the later

stages of disease, and progress demands that the predisposing and early stages should be investigated with equal thoroughness and energy. In order to do this other fields must be explored, other methods used, and other individuals employed, than have served the purpose of investigation and teaching in the past.

CHAPTER I

MEDICAL EDUCATION

THERE has been of late years, a movement amongst all who are concerned with medicine, to reconsider its teaching. There have been, for instance, the elaborate investigations by Abram Flexner on the teaching in the schools of America and Europe. There has been a Royal Commission on University Education in London, to inquire, amongst other things, into the best methods for instruction in medicine, and evidence has been given before this Commission, by many of the leading authorities in medicine in this and other countries. Numerous addresses and lectures, such as those contained in a volume recently issued by the Edinburgh Pathological Club, have been given by teachers and others, so that it might be assumed that all that can be said on this subject has been said and that the experience of these diverse authorities ought to be sufficient to guide the instructors of future doctors on the best lines. There are, however, factors in education that have been left out of account, and with some of these I will deal. Some are simple and seemingly unimportant, but it is often the simplest matter in medicine which is least understood. Everyone seems to realise a defect in teaching, but the nature of the defect is not recognised, because of an imperfect conception of the aim of medicine, and of the neglect of the simpler experiences of everyday life—experiences which, if understood, would reveal where education fails.

The Student, the Teacher and the Examiner.

As one of the purposes of this book is to show that there are fields, which are essential to research, which the general practitioner alone has the opportunity to cultivate, it is necessary to discuss his education. Before dealing more specifically with this subject, there are one or two preliminary matters which, though simple, deserve consideration.

When the average student enters on his medical studies, he is embarking on a new phase of education, usually conducted on lines different from that which he followed in his school days. All is strange to him, and he does not know which is the best method to follow. As much of his instruction is given by lectures, he assumes that the words that fall from his teacher's lips should be carefully remembered, and he attempts to take notes of the lectures, as faithfully as his ability to write rapidly will permit. These notes, being hasty, need revision, and he spends hours in the evening trying to decipher them, or, it may be, to transcribe them. As most of what he has heard is new to him, and words are employed whose spelling and meaning he does not know, his studies are made very laborious.

Such was the experience in my student days. Since then, many teachers have recognised these difficulties, and have adopted methods which render instruction much easier. It has always seemed to me, however, that some attempt should be made to indicate to students the best way their studies should be pursued. When left to himself, a student may adopt some plan, which entails an unnecessary amount of fatigue and waste of time, and his student course may be run before he discovers for himself some other method which helps him to acquire the necessary knowledge with greater facility. If the average student devotes

his whole energy to his task, he will discover that by the end of half an hour his mind has become exhausted, so that he cannot remember the new facts, or follow the reasoning as well as when he started, and that a continuance of his study will result in reading in a listless fashion, and getting so imperfect a knowledge of the subject that he will be forced to read the subject over repeatedly. This of course takes for granted that, while he is studying, he concentrates his attention upon the matter in hand.

After half an hour or so, therefore, he should rest, or do something to divert his attention for fifteen or twenty minutes, before tackling another subject; he should then pursue the same strenuous study for another half hour.

This suggestion may seem simple, and some students may be able to carry on their studies longer and in a different manner, and others may have a better method. Personally, I found it very useful and I have advised students to follow it, and they have found it very satisfactory. I mention this in order that teachers may consider the matter, and may be able to give modifications of some plan, more suited to their particular subject.

Another point of great help to the student is, that he should have some knowledge of the subject of each lecture before he hears it. This perhaps, is a matter more for the teacher. My recollection goes back to my student days, with a sense of distaste for the boring fatigue of taking notes (and transcribing them) on a subject of which I was absolutely ignorant; moreover, I had the mortification to find in later years that all the matter I had written so painfully and imperfectly was already printed in books. Among these lectures, a few stand out for the pleasure there was in attending them. I had been fortunate in getting very full notes

of these from students who had previously heard them. Each morning, before going to the lectures, I read them carefully over, and so, during the lecture, could sit and listen with an intelligent interest, and appreciate the demonstrations, undisturbed by the necessity of taking hurried notes.

It always seemed to me a curious arrangement that teachers should expect their students to write down their utterances when the matter was already printed in books. The taking of notes is bound to distract the student's attention, for the reason that he cannot give his whole attention to understanding what the teacher is saying, and so unconsciously leaves the understanding of the subject to the time when he can read his notes. If the teacher, for five minutes before the end of his lecture, gave an outline of the subject of the morrow's lecture, and told the student in what books to find the matter, the student would next day come prepared to appreciate the lecture, and to understand the demonstrations, and to note when the teacher had supplemented the text-book by new material.

In this way the teacher would have an interested and intelligent audience, quick and ready to appreciate the essential points, and be stimulated by the fact that he was being so intelligently appreciated. The benefit to the student would be very great, not only in that the subjects would be rendered far more interesting, but also in that he would be saved an unnecessary amount of time and fatigue.

An allied matter, in which a few comments may not be out of place, is that of the examinations. Everyone recognises the unsatisfactory nature of examinations, but so far it has proved impossible to find any other way of determining whether or not a man is qualified to practice his profession. No doubt the majority of examiners recognise the purpose of examinations, and

conduct them in a sensible and reasonable manner, but the random way in which examiners are selected, with no reference to the capability of a man, as examiner, frequently results in men being appointed who are totally unfit for their posts. Such men do much harm and injustice. The purpose of an examiner is to find out how much the student knows, and most examiners recognise this, and are skilled in putting the questions in such a manner as to discover the extent of a student's knowledge. There are examiners to whom this idea has not occurred, and they seem to delight in questions that bewilder the student. Often, no doubt, this is done thoughtlessly, but if the examiners realised the intense anxiety of the student to pass his examination, and how much his future life depends on the results, he would see that a question put lightly, and which he might not expect properly answered, could so disturb the student, that his wits might be lost for the time being, and his answers to other questions not given as well as if he had not been flustered and upset. Every one of us can recall incidents of that sort. Some examiners are not only stupid, but are almost malevolent, and seem not to desire to find out what the student knows, but to delight in bamboozling him.

Some protection should be given to the student against this type of examiner. Questions ought to be taken down, and if questions are asked which show the examiner to be unfitted to find out what a student knows, he should be summarily dismissed, and the students who have failed should be re-examined.

The majority of students devote themselves sincerely to their studies, and are anxious to acquire a knowledge of their profession. When at any examination a big proportion fails to pass, it may be taken for granted that the fault lies either with the teacher or the examiner,

and the questions put, and the answers given, would furnish a clue as to which individual was at fault. The students have no one to stand up for them, and those in authority ought to recognise that students should not suffer for the faults of their teachers and examiners.

Influence of tradition on teaching.

I desire it to be understood that in the criticisms I give here I am not casting reflections on any individual. We are all creatures whose mode of thought is influenced by tradition and environment. Teaching, and particularly teaching in medicine, is more affected by tradition than almost any other subject. It was begun in the dark ages when the ignorance was profound, and although it is the agent by which ignorance is being conquered, yet it has a fashion or habit that is continued from these dark ages to the present time. There are handed down, from one generation to another, conceptions which influence the methods in teaching; for the pupil but imitates the master, and though he may introduce certain modifications, the central idea still remains, and newer methods may be but the reaction of a fashion inspired by an imperfect conception of the science of medicine. I do not say that all traditions are bad, but it behoves us, when methods are being pursued which originated in bygone times, to pause now and then to consider if these methods are quite suited to their purpose. Therefore, the criticisms I shall make have reference to the conceptions which have come down to the teacher, and the criticisms are not levelled at the individual, but are made to show that a different method of looking at the subject reveals features which have been overlooked.

Nature of the experience necessary in a teacher.

There is an important idea which has been omitted

in the consideration of medical education, namely *that a teacher of practical matters must be one who has experienced what he teaches.* We all recognise that the best teacher for a youth who wants to be a shoemaker is a man who is in the habit of making shoes. If he wants to be a chemist, he goes to a teacher who has a practical knowledge of chemistry. That idea in relation to these subjects will be accepted universally. Unfortunately, this common sense idea is rarely applied to medical education. The vast majority of students who enter on the study of medicine ultimately become general practitioners, and yet a student may pass through his curriculum and be instructed for years by a large number of teachers, not one of whom has had any experience of the life he is to lead as a general practitioner. As a result, a large portion of the student's time and energy has been spent in acquiring information that is of no use to him in the practice of his profession, while much of the knowledge which he often finds essential has never been given to him.

This assertion may seem so extravagant as scarcely to require serious consideration. I shall shortly show the grounds on which it is given, but here I would call attention to one striking fact which should give cause for reflection.

The General Practitioner who has experienced and suffered from a defective education is never consulted in educational matters.

When any attempt is made to modify the instruction necessary for the general practitioner, every kind of individual connected with education is consulted—except the one individual capable of showing from his own experience where medicine fails—i.e., the general practitioner himself. Every thoughtful general practitioner recognises that there were great defects in

his education, which he discovered soon after entering on the practice of his profession. Moreover, he meets with phases of disease of vital importance to the study of medicine, which were never referred to in his medical education. So lacking is the perception of what are the essentials required for medical education, that the Authorities who arranged the evidence before the Royal Commission did not deem it necessary to call one general practitioner to testify before them to what extent his education had fitted him for his work.

If we turn to the consideration of the subjects he is taught, it will speedily be realised how much of his time and labour are spent unprofitably. In his training he is taught a variety of subjects which tradition has imposed upon the teaching world as being necessary to his education, such as botany, zoology, and chemistry. It may be said that a knowledge of these subjects is necessary to enable him to understand and appreciate the facts in his other studies, such as physiology and clinical medicine. Admitting this to be true, how much is necessary for this purpose? The teachers are men skilled in their particular subject, but have little knowledge of what part of their subject is necessary and what part is immaterial to one who does not intend to become a botanist, a zoologist or a chemist, but a practitioner of medicine. Seeing that these subjects are of a vast extent, and that the medical student can only acquire a knowledge of a small part, it is not unreasonable to expect that his instruction should be limited to that portion which will be of real use to him in his studies and in the practice of his profession.

Necessary subjects should be taught with the definite perception of their relation to the practice of medicine.

(A.)—*Anatomy.*

There are certain subjects which are fundamental for

the acquisition of medical knowledge which should be studied with a fulness involving considerable detail, such as Anatomy. But even here much simplification of knowledge and re-arrangement is desirable. The instruction in anatomy, as in so many other subjects, is often but an unintelligent description of facts, so that the student is burdened with an accumulation of many trivial details, of little moment, and these are given with so little reference to their use that he cannot discriminate the non-essential from the matters that will be of the first importance in his future studies and practice. Anatomical books and teachers of anatomy give as much or more prominence to a detail, such as a tuberosity on a bone, which will never be referred to again in the student's life, as to some vital point like the centre of respiration. This lack of perspective in teaching, whereby the immaterial is given the same prominence as the essential, is one of the greatest defects in every sphere of medical education. This aspect is being realised, and already some anatomists endeavour to put the subject before their students in a way that takes into consideration the significance of the structures.

(B.)—*Physiology.*

In physiology we have a subject of immense importance to the future practitioner. The subject is such a vast one, and its different branches are capable of such elaboration, that it is difficult to know how to teach a student, who does not intend to become a physiologist, just that portion of the science that would be of service to him in the practice of his profession. It is with such a subject that a teacher should have some understanding of what the experience of his students will be. It is manifest that the teacher can easily give lectures on the special branch of physiology

in which he is himself interested, but which may have little or no bearing on the experiences of his students in their future career. On the other hand, he may leave out much that would endow his students with the power of seeing and understanding phenomena in their patients which would be invaluable to them in applying their knowledge for the relief of suffering, and of guiding them in the pursuit of knowledge for the advance of medicine.

There are two broad aspects of the science of physiology which ought to be taken into consideration—the academic and the practical. In the former the physiologist pursues his researches to find out the laws that govern the activities of nature, and this is pursued altogether apart from its practical use. Not that a practical use may not arise, for it is assumed, no doubt with good reason, that the revelation of the laws of nature—though at the time their application to practical purposes may not be clear—may be utilised by those interested in their practical application, inasmuch as they can turn to these academic studies for the knowledge that will enable them to achieve their object. It may therefore be said that the academic pursuit of physiology, apart from its immediate application, is ultimately of use, inasmuch as those, who seek to understand the manifestation of disease, can find in these researches the data concerning the phenomena of healthy function.

That represents the attitude which justifies the physiologists, barren of clinical experience, in teaching students of medicine. While there is much to be said for it, is there not a better means of teaching physiology to men who do not wish to become academic physiologists, but practitioners of medicine? Physiology is a very wide subject, and it is impossible for any one physiologist, let alone a general practitioner,

to comprehend all its diversities. Seeing then that only a portion of the subject can be taught, why not limit the teaching of physiology to those parts that will shed light on the problems which will confront the practitioner of medicine? Surely it is as truly scientific to find out the laws that govern the manifestations detected in the human body, as it is to find out those, knowledge of which is only to be obtained by some recondite experiment in the laboratory. The physiologist professes to teach the functions of organs; then why not let him teach those functions which confront the general practitioner in his work? This is a point which, one is glad to say, some physiologists have realised, and there are now a number of physiologists who do direct their teaching towards matters which have a bearing on the manifestations in the human body. Moreover, physiologists are associating themselves with clinicians in the investigation of morbid processes. This tendency should be steadily developed till the physiologist becomes a frequent visitor to the medical and surgical wards, helping in the interpretation of the phenomena of disease and gaining a conception of the problems with which his students will be faced. In this way he can obtain a definite aim in the prosecution of his researches in his laboratory, and may then be able to teach a human physiology.

(C.)—*Pharmacology and Therapeutics.*

At the present time it is hopeless to expect in any school of medicine that the student will acquire a sensible understanding of this subject. He may attend the lectures delivered by a pharmacologist of the highest repute; he may work in laboratories where the action of remedies is studied with such degree of accuracy as the present state of the subject permits; he may even have instilled into him the principle on which

different drugs act on the organs, but as soon as he passes into the practical application of this part of his education in the different departments of clinical medicine and surgery, he will find all his theoretical knowledge thrown to the winds. In every clinical department, general or special, the student will find his teachers using a great variety of remedies for purposes never heard of in his pharmacological studies. Remedies will be prescribed, not because they have a demonstrable effect, but because the teacher has a belief that they possess some property or other. He will thus see remedies prescribed on grounds of faith and not of reason. Not only that, but in subjects where there are a number of teachers, as in clinical medicine, a series of remedies for one complaint will be employed by one teacher, while for the same complaint other remedies are given by other teachers.

It is easy to understand what a bad effect this state of affairs has on the impressionable mind of the student, and its effect is reflected in the practice of his profession. On the one hand he sees his teachers so frequently prescribe remedies because of an unreasoning belief in their properties, that he becomes sceptical of the influence of any drug, and in his future practice he restricts his use of remedies to a few drugs whose effects are demonstrable and some simple medicaments given for the purpose of pleasing the patient or acting by suggestion. Other minds may be differently affected, and may carry away the belief that for each symptom of disease there is a particular drug, and as years go on add to their armamentarium, so that at last they employ a formidable series of drugs. Others again try to be up to date, and use only the remedies that are the fashion for the day—the leaders of fashion being usually the owners and advertisers of proprietary preparations.

What is the remedy for this state of affairs? It is that no remedy be prescribed in the hospital unless the teacher who prescribes can demonstrate to the student its reaction on the human body. Each time a drug is given the teacher must give the reason for presenting it, and the student must be set to watch for the effects it is supposed to produce. Such a practice would have a most beneficial effect upon the progress of medicine. It would compel the teacher to study his subject with greater accuracy, so as to find out the real nature of the symptoms, and their susceptibility to his remedies. It would train the student in the habit of careful observation on the human subject, and it would reveal to him phenomena that would otherwise escape his attention. In his future career he would not only prescribe for his patient with greater intelligence, but he would be trained to observe the results of treatment, so that in time an accurate knowledge of the action of remedies would replace the present chaotic state.

(D.)—*Clinical Medicine.*

The chief advance in general medicine in the last 50 years has been a study of the damaged tissues after death, and the recognition of the symptoms these damaged tissues provoked during life—what are called physical signs. A physical sign, it is to be noted, as a rule is due to alteration of structure, so that disease, after it has damaged the organism, has been the main object of study in the past. No doubt this was a very necessary step, but having achieved it, the next advance must be (1) the recognition of the diseased state before it has produced gross structural change, and (2) the condition that predisposed to, or induced, the disease.

It must be recognised that laboratory methods

render little help in detecting disease at the early stage. Such means as the X-rays may reveal disease when it has destroyed or altered structures. The microscope may reveal a germ that plays an active part, but it cannot reveal the symptoms which the germ produces, nor the conditions that favour the entrance of these germs, nor indeed, the fact that the man is ill. It is the physician who is called upon to recognise the symptoms provoked in the early stages.

The great number of patients who feel the signs of ill-health, which indicate the onset of a disease, consult the general practitioner. The patients consult him in the confident belief that he will detect the cause of their complaint and cure it.

It will thus be seen that it is a teaching physician's duty to instruct the student how to recognise the early stages of disease. Let us see how this is done.

To begin with, it must be understood that the signs of disease in the early stages are the most difficult to detect and to understand. They are mostly subjective phenomena, and it requires great experience and skill to obtain from a patient a coherent description of his sensations. Moreover, the basis of these sensations, the mechanism by which they are produced, requires a very profound knowledge of the physiology of the body —a knowledge which can only be acquired after years of observation and training. To obtain this knowledge, the life history of a disease must have been studied and all the variations in the symptoms noted, and the gradual evolution of one sign after another. The results of surgical operations, and the post-mortem findings must be studied and co-related to the symptoms during the life of the patient.

It will thus be seen that a long preliminary training is necessary before a teacher can become competent to instruct students in the detection and in assessing the

value of the symptoms produced by disease in its early stage. It will also be realised that this stage, from the patient's point of view, is the most important, for it holds out the possibility of cure before his organs have been damaged.

What are the methods adopted to instruct students in the early stages of disease? In the hospitals, where all instruction in clinical medicine is given, the patients in the early stages go to the out-patient department. Common sense would say that here, where the signs of disease are the most difficult to make out, and the hope of cure is at its highest, the most experienced physicians would be employed, and that all the aids of laboratory technique would be at hand to help in the recognition of the disease. In no teaching institution is this ever done. Here, instead, is placed the youngest member of the staff, lacking in experience, ignorant of the meaning of the signs, and incapable of eliciting them. He searches carefully for physical signs, and if there are no physical signs the patient is either discharged with a bottle of physic, or told to return from time to time until a physical sign is discovered; then, and not till then, is he sent to be under the care of the skilled physician; and then, and not till then, does he receive the assistance of laboratory methods in his examination.

On the other hand, in the wards, where disease has advanced so far as to produce physical signs or other demonstrable signs of disease, mostly easy of recognition, we have the trained physician, the research student, and all the paraphernalia of laboratory assistance.

This description will be contested by some as an untrue or exaggerated picture of the way things are managed in some particular school or hospital, and I admit that exceptions exist here and there; yet it is

sufficiently accurate to be of use in focussing attention on the defects of medical education. I may say I have visited hospitals and medical schools in many countries. I have had shown to me with pride splendid pathological institutes, excellent wards with laboratories attached, replete with all kinds of mechanical devices and presided over by competent men who have demonstrated to me a great varicty of apparatus for detecting some phases of disease; but I havc never yet been asked to see an out-patient department, nor shown any place where attcmpts to study the symptoms of disease, in its early stages, were being carried on.

CHAPTER II

MEDICAL EDUCATION (*continued*)

Scientific Education.

THERE is an opinion widely current in educational quarters, that the teaching of medicine has become of late years more "scientific," and that the student when he qualifies is a more scientific doctor than he was 30 or 40 years ago. Much as it may surprise people, this opinion is unjustified. It is assumed, for instance, that the teachers are more scientific and each has a better grasp of his subject, because he has been trained more thoroughly and has concentrated his attention more specifically upon one subject than was the case 30 or 40 years ago, when each particular science was not so well advanced, and when the teacher had to instruct in more subjects than one.

While the splitting up of medicine into separate branches has much to commend it, it is not without certain detrimental effects upon education. The chief defect is in the fact that there is no teacher with a broad outlook on medicine, one who can see all the different branches in their proper perspective. Fifty years ago progress was being made upon certain lines which tended to a clearer conception of what medical education was, because the teachers were men who had to take a broader outlook. At the present day, there is not a single teacher in a school of medicine capable of taking that broad outlook. Each one has his particular department. As the student passes from one

section to another he finds each exponent magnifying his own department, and in his innocence the student cannot distinguish the more important from the less.

Different Ideals in Medicine.

Fifty years ago the teachers were, many of them, men of wider views. In Edinburgh University the professors of clinical medicine were also professors of pathology, physiology, therapeutics and public health. It can be readily understood what a width of knowledge would be brought to bear on the clinical phenomena by men who were proficient in other subjects, and also how, in teaching their special subjects of pathology, physiology, therapeutics, and public health, their intimate knowledge of clinical medicine would give a width of view of real practical importance to their lectures. I can say with conviction that the best clinicians that it has been my good fortune to meet, and who inspired me with the greatest interest in clinical work, were those professors whose sanity of judgment, coupled with a profound knowledge of allied subjects, gave me a far wider outlook than the teachers of to-day are capable of doing. I readily confess that in those days we had not such a smattering of many things as the present graduate, but I am certain we were better qualified to enter upon the re-education which became necessary when we started the practice of our profession in places remote from the seats of learning.

In contrasting the past and present methods, my idea is not to advocate the old methods, but to emphasise the fact that there are two ideals of method in medicine, which, though they have the same aim, yet seek to achieve it by different paths. The dominant ideal at present, if carried out to its fullest extent, is bound to lead astray. I believe that another ideal

exists and that it can be demonstrated to be the one which will open a path far more likely to reach the goal of medicine. The spirit which has been for centuries characteristic of the British School, would urge us to study the human individual for the signs of disease, and, problems having been perceived, to seek their solution by experimental and other laboratory methods. The physician, inspired by this ideal, seeing in his patients a series of phenomena, tries to interpret them; failing to do so, he then seeks himself, or directs others to search, for an interpretation. He studies the individual as a whole, recognising not only the more dominant signs of illness, but seeking for the more subtle signs which can only be revealed by the trained senses of a skilled examiner, or by his intelligent questioning of the patient based upon an understanding of the significance of the patient's sensations.

The ideal which is dominant to-day, and which has to a great extent superseded this other, depends on the revelation of the signs of disease by some mechanical contrivance devised in the laboratory. I am aware that no one will admit that he supports any method which does not take into account all the features of disease, and indeed, those who employ the laboratory methods, maintain that they do this in a more thorough manner than those who do not use these methods. It might seem that there is then no difference in the ideals, and that those who employ the seemingly more scientific methods are more likely to advance medicine towards a science. Indeed, so plausible is this way of looking at the subject, that it has won all along the line, and in many schools its development has proceeded to an extraordinary extent; other schools which have not advanced so far, feel that they are left behind and urgently call for an extension of these methods.

There is, however, a very important distinction

between the two schools which I will later develop. To make my meaning clear, take one aspect of the question, which shows a great defect in our knowledge. In any given case when a subjective symptom or a physical sign is detected, it is imperative, in addition to finding out the mechanism of its production, that its value should be assessed in relation to its effect upon the patient's future. As it is by signs or symptoms that disease becomes manifest, the question arises, does this sign or symptom foretell the on-coming of a disease that will impair the health? Does it call for treatment or is its cause amenable to treatment? Common sense tells us that these questions are of the very essence of medicine, and are the foundation of all practice. To a very great extent the knowledge required to answer these questions does not exist, yet they represent the vital problems that must be solved if ever medicine is to become a science. It is indeed deplorable that present-day medicine has not realised their importance and, so far from answering them, has not even yet recognised what methods should be adopted in order to find answers. This, however, need cause no surprise, for it will be found after a little thought, that the diversion of research into the laboratory field, and the breaking up of medicine into sections, has rendered the solution of such vital problems an impossibility.

It is manifest, for instance, that if we are to understand the significance of any symptom, particularly its bearing on the patient's future, we must watch the individual showing it for long periods—it may be for years—to find out what will happen if the condition is left untreated, and further, to find out whether or not treatment may modify its course. It is manifest that this cannot be done in laboratories or in hospital wards. The opportunity naturally lies with the in-

dividual who sees the whole course of the illness—that is, the general practitioner. As the progress of disease in one organ invariably reacts upon other organs, it is further manifest that a specialist who limits himself to his department has no opportunity of seeing the various phases of disease, and therefore can never satisfactorily assess the value even of these symptoms which are peculiar to his speciality.

It will thus be seen that the present-day attitude fails not only to recognise the only individual who has the opportunity for the solution of this vital problem, but fails to recognise the methods necessary to be employed for its solution.

Illustration of the effects of defective education shown in the Recruiting Problem.

Of the vast importance of this matter of assessing the value of symptoms, and of the disastrous effects of this lack of knowledge we have had recently striking evidence. When at a time of great national stress, a call was made for men to join the army, it was assumed by the people, as well as by the military medical authorities, that there existed sufficient knowledge to separate the fit from the unfit. This was really a test on the greatest scale of the adequacy of medical teaching. At the outset the profession failed, for large numbers of perfectly healthy men were rejected, because they presented signs whose value the medical examiners were unable to assess. Later, when the national strain became greater, and it was necessary that even those with impaired health should enter military service, the military authorities again thought they possessed the knowledge necessary to say what amount of physical effort a man was fitted for. It is unnecessary to dwell upon the complete failure in the recruiting. So glaring were the mistakes, so unjust were the decisions, that the whole nation was aroused,

and Parliament was forced to undertake an inquiry. The result of that inquiry was that the medical examination of recruits was taken out of the hands of the military and placed in the hands of the civilians.

The real cause of all this disturbance was nothing more nor less than the fact that those in authority had never understood the significance of symptoms. They had unwisely assumed that it was a matter so simple that any qualified man possessed the necessary knowledge. It never dawned upon them that to assess the value of symptoms was one of the most difficult tasks and required a training which few obtain, and opportunities which few teachers ever possess. There is no doubt that among general practitioners a large amount of this necessary knowledge has been obtained as the result of long and painful experience. Their knowledge is, however, personal, and of that vague, indeterminate kind which is difficult to express clearly. Yet it was this knowledge which helped to save the situation.

The outcome of laboratory ideals in teaching.

The complexity of medicine renders it difficult to perceive which is the best course to pursue. One may reason from some limited experience, and adopt a certain procedure, and find the logical carrying out of that procedure leads one astray. The outcome of the laboratory ideals which are dominant was not perceptible when these were first adopted. To-day we can see where they tend. I hope later to demonstrate that these ideals result not only in a limitation of outlook, but by replacing the real clinical methods they destroy the efficiency of teachers in the examination of their patients by shutting them off from personal contact with the evidences of disease. Methods which are essential but are purely clinical have fallen into neglect.

I wish to point out how detrimental these concep-

tions prove in the education of the student. It is far better to be trained to understand a few matters thoroughly than to have a superficial knowledge of a great many things. This dictum is peculiarly applicable to the examination of patients, as will be shown later. The tendency to-day is to extend the use of mechanical methods so indefinitely that no one can acquire a knowledge of more than a fraction of those now in use. Students are now compelled to learn all these new methods in some schools, and I give below the subjects recommended in one school for the student's study during three half-day exercises per week for six months. When it is understood that even the specialists who employ these methods have only an imperfect knowledge of the meaning of the signs which their method brings to light, the practical benefit to the student can be inferred. I need not dwell upon the state of the student's mind at the end of a six months' course of this kind of instruction, especially when this subject is probably but one of several others pursued in the same spirit.

The syllabus to which I have referred is a recommendation by the Professor of Clinical Medicine at Johns Hopkin's University, Baltimore, a school of medicine looked upon by many as one of the most advanced seats of learning, not only in America, but in the world, and rich in its endowments for the study of medicine.

"During the first year of clinical work, the student should also acquire *the technic of a whole series of special and instrumental methods of examination.* Thus, in my opinion, every student should at this period of his growth become acquainted with Rontgen-ray apparatus, and the technic of roentgenoscopy and roentgenography as applied to the study of different parts of the body. In the clinical, bacteriologic and immunologic laboratories, he should learn the clinical applications of bacteriologic methods (collection of materials; diagnostic examinations by microscopic and cultural methods, or by animal inoculations and virulence tests),

and the application to the clinic of the doctrines and technic of immunology (clinical studies of agglutinins, bacteriolysins, hemolysins, precipitins, opsonins and ergins), with especial emphasis on, say, the Widal reaction, the Wassermann reaction, the Schick reaction and the tuberculin tests. Next, might come training in special methods of studying the respiratory apparatus (rhinoscopy, pharyngoscopy, transillumination of the paranasal sinuses, laryngoscopy, a demonstration of the use of the bronchoscope and of exploratory puncture of the pleural cavity); such studies, supplementing the course in general physical diagnosis of the lungs and pleuræ, the course in the examination of the sputum and the course in roentgenology of the thorax, bronchi, lungs, pleura and diaphragm, will be the best possible preparation for the investigation of the special diseases of the respiratory system to follow in the last year of the student's course. Similarly, the special methods useful in investigating conditions in the circulatory apparatus may now be rapidly acquired. Roentgenoscopy of the cardiovascular stripe, so helpful for examining the configuration of the heart and in the recognition of aortic dilatations, will present no difficulties to our clinical student, and he will be fascinated by the simplicity and precision of teleroentgenography, which has largely replaced orthodiagraphy and which serves as a salutary control of the results obtained on percussion of the relative cardiac dulness.

"Even the precise methods of mechanical registration of the movements of the heart and blood vessels (sphygmography, cardiography), of the heart sounds (phonocardiography), of the electrical currents generated in the heart during its activity (electrocardiography), and of the pressure of the blood within the arteries and veins (sphygmomanometry or tonometry of the blood-vessels) can be speedily acquired, for the student has already had at least a glimpse of them in the laboratory of physiology. Though as yet we do not know how to value the results clinically as well as we should like, the methods that have been devised for determining the functional capacity of the heart should be demonstrated. The student thus trained at the beginning of his clinical studies in the special methods of clinical angiological examination, in addition to the ordinary physical methods, should have no difficulty in his later studies in accumulating the necessary data for forming a diagnosis when confronted by a cardiac arrhythmia, an inflammatory or a degenerative cardiopathy, or a hypertensive arterial malady.

"Turning to the special methods useful in investigating the digestive system, the student has a considerable technic to acquire in addition to the ordinary physical methods of examination of the viscera, and the laboratory studies of the secretions and excretions. Thus, instruction should be given in the methods of examining the teeth and gums preferably by a dentist attached to the clinic. Dental caries, paradental infections with formation of blind abscesses at the roots of teeth, and pyorrhea alveolaris are now so

important, not only for themselves, but also in their bearings on disease elsewhere in the body that students dare not be permitted to leave the medical school without knowing how they may be recognised by inspection, by percussion, and by special roentgenograms on dental films, so that dental aid may, when required, be obtained. Then the newer technic of examining the esophagus should be demonstrated, though it may not be possible to give the undergraduate student actual practice in the passage of esophageal bougies, in roentgenology of the esophagus or in esophagoscopy. The physical exploration of the abdominal viscera will be taught in the general course on physical diagnosis.

"Actual practice in gastric intubation of the fasting stomach and of the stomach after a test diet, and actual experience in roentgenoscopy and roentgenography of the stomach and intestines after a contrast meal and a contrast enema, should now, in my opinion, be required of all students. The roentgenologist of the clinic should have a large demonstration room that students may visit; there they should see typical normal and pathologic roentgenologic findings serially displayed; moreover, a few systematic demonstrations of these should be made by the roentgenologist to the class, so that every student may become familiar with the roentgenographic appearances of conditions like idiopathic dilatation of the esophagus, filling defects due to ulcer, or carcinoma, of the stomach and duodenum, intestinal stasis, kinks, adhesions, and other forms of intestinal obstruction, diverticula of the sigmoid, etc., and will know how to make use of the Rontgen-ray method for recognising them.

"The special methods of studying the pancreatic functions by examination of the duodenal contents (obtained by the duodenal pump), the feces, and the urine will require but little time; the same applies to the special methods of examining the liver and the biliary passages and their functions. Instruction in digital exploration of the rectum and demonstrations of proctoscopy and of rectosigmoidoscopy should form a part of the course.

"As regards the urogenital system, its examination dare not be omitted in the teaching of clinical medicine. This part of the body should be systematically examined, as is every other part, for otherwise conditions of great importance for the general medical diagnosis frequently will be overlooked. It may be desirable, however, for obvious reasons, to have certain parts of urogenital methodology taught in the surgical and gynecologic clinics. The teaching of methods for examining the urine, of physical and roentgenologic methods of examination of the kidneys, and of methods of testing the capacity of the kidneys to excrete certain substances, belong to the medical clinic; and if, for any reason, the other clinics do not demonstrate urethroscopy, cystoscopy, ureteral catheterization, pyelography, etc., the medical clinic would have to provide for this teaching.

"As to the special methods of examination of the bones, muscles

and joints, only brief instruction will be necessary in the medical clinic, since by custom those methods are usually very extensively taught in the surgical clinic, especially in its orthopedic subdivision. For a rounded view of clinical medicine, however, some attention to them is necessary in the medical clinic where examinations for pain, limitations of movement, Rontgen-ray examination, trichinæ in muscles, etc., may often have to be made. The examination of the skeleton is often very important for the internist as throwing light on the metabolic functions and especially on the functions of the endocrine glands.

"The teaching of neurologic and psychologic methods of examination should occupy enough time to enable the students to acquire competence in at least the main procedures of clinical medical inquiry. It is best to divide this work into three parts, the first part dealing with the methods of accumulating neurologic and psychologic data from the patient, the second part dealing with utilisation of the accumulated data for deciding on the site of any lesions or of any abnormal processes present in the nervous system, topical diagnosis, and the third part dealing with the considerations that permit the drawing of inferences regarding the nature of the lesions or of the pathologic processes. Thus, in the first place, the student will be taught how to make accurate examinations of the senses and of the sense organs (cutaneous, deep, gustatory, olfactory, acustic, vestibular and visual); of the motor functions and the reflexes; of the co-ordinating powers; of the capacity for speech, for writing and for other complex movements; of the functions of the smooth muscle and of the secreting glands; of the sphincters; and of the trophic functions.

"In this connection, certain applications of anthropologic methods of measurement may be practised, as well as the technic of roentgenologic examinations of the nervous system, skull and spine, that of lumbar puncture, and that of diagnostic electrical examinations of the muscles and nerves. He will be taught at this time, too, how to examine the mental state of a patient, paying attention not only to the patient's consciousness as a whole, but also to the special powers of attention, of perception, of identification and of diction, to the affective life of the patient as revealed by his feelings, emotions and moods, and to his conative functions, often called 'the will,' and judged of by the person's behaviour or conduct.

"The second part of this instruction in clinical neurology will involve a review of the architectonics of the nervous system and of the psychology of the several nervous systems (centripetal, centrifugal and associative), in as far as these subjects can be applied to localizing diagnosis; the student will quickly see the reasons for deciding whether the lesions present, or the pathologic processes going on, concern the peripheral nerves, the spinal cord, the medulla, pons or cerebellum, the midbrain, the interbrain, or the end-brain, and whether they are focal or diffuse, single or multiple.

" And in the third part of the neurologic work, instruction will be given in the principles on which the diagnosis of the nature of a nervous disease is arrived at. The difference between the so-called ' organic ' and ' functional ' diseases of the nervous system will be discussed, and the criteria for recognising whether a given organic disease has been due to disturbances of development, of the blood supply, or of the nutrition, to toxic or infectious processes causing degeneration or inflammation, to trauma, to parasitic invasion, or to tumour growth, will be established.

" Instruction in methods should include finally the procedures used for the clinical study of metabolism. After a brief review of the physiology of metabolism, the student should be taught the requirements of systematic metabolic studies. Though there may not be time to do actual practical work in the quantitative chemical analysis of foods and excreta, the organisation of a modern metabolic study will be illustrated and the students will become acquainted with the manner of preparing a patient for such a study with the periods of observation required, with the doctrine of ' balances,' and with the preliminary tests that may have to be made of assimilation, digestion and absorption. After this introduction, the methods of determining in man the metabolism of proteins, nucleins, and purins, carbohydrates, fats, water, mineral substances and vitamines will be demonstrated. The different forms of apparatus for direct and indirect calorimetry will be described and the use of at least some of them actually demonstrated. Such a preliminary discipline in the practical-technical methods of metabolic study I regard as essential if the students are later in their course to proceed to the study of states of under-nutrition and over-nutrition, of the several amino-acid diatheses, of diabetes mellitus, and of gout, armed with the knowledge and technic that the science of medical diagnosis has now made available. Teaching hospitals should take the lead in making suitable provision for these studies of metabolism that are now indispensable for satisfactory diagnostic and therapeutic work.

" As an appendix to the doctrines of metabolism the methods of investigating the disturbances of functions of the endocrine glands, so interesting at the present time to all workers in internal medicine, should be taught. Aside from certain pharmacodynamic tests to be made with epinephrin, atropin, pilocarpin, etc., judgments regarding the activities of the several endocrine glands depend largely upon (1) observation of the general exterior of the body (facies, height, bony skeleton, span, skin, hairs, mass and distribution of subcutaneous fat, shape of pelvis, appearances of the acra, and of the genitalia, teeth); (2) systematic metabolic studies; and (3) systematic studies of the functions of the autonomic nervous system. The main diagnostic facts in this active area of clinical medicine can be quickly assembled and given in concise form to the students; thereafter, they may apply them in their work in the wards to the analysis of endocrinopathic cases."

CHAPTER III

THE SPECIALIST IN MEDICINE

The origin of the Specialist.

The effects of disease, on its invasion of the human body, are seldom limited to one structure or organ. There is thus produced a series of phenomena of great variety, the variations depending upon the peculiar reaction of the different organs and tissues affected. Not only is a series of phenomena produced in this way, but the functional disturbance of one organ reacts on the function of all the other organs, so that from a simple cause we may get a bewildering variety of symptoms.

It has been the practice of medicine to select for consideration the symptoms that were most prominent, and to associate the disease with the organ that exhibited those symptoms. The less evident symptoms received a mere passing notice or were ignored or undetected. Notwithstanding this, the symptoms perceived are so numerous that for their elucidation it is now considered necessary that men should devote their attention to one particular organ, or one particular disease, or to the employment of one or more technical methods in examination. Considering the different ways in which an organ may be disturbed, there is no lack of phenomena for observation in the closer study of any one organ or by the use of mechanical devices. Consequently the specialist even in re-

stricting himself to his limited field, has plenty of material for observation, material which can be increased indefinitely by the exercise of some ingenuity in devising new means and methods for detecting symptoms. A glance at pages 35 to 39 shows what an enormous number of devices have been evolved in late years for the detection of symptoms, and there seems to be no limit to their increase, as new methods are continually being added to the already formidable list. Each new method is looked upon as an evidence of progress, and as the employment of instruments is supposed to give a scientific accuracy to the observations, it is imagined that in this way medicine is becoming more scientific.

To employ these numerous methods men have to be trained along special lines, so that we get groups of men set apart for these purposes. The concentration of their energy in their particular departments leads to a continual development of methods. It is but natural that each specialist should strive to advance his own particular branch, and it is also but natural that he should seek to magnify the importance of his particular department. The result of these procedures is that there are now a large number of specialists, each employing different devices for the detection of the symptoms of disease, and there is being brought to light such a bewildering variety of phenomena, that no single individual can fully comprehend them.

Specialism in practice.

It is interesting to study the outcome of this conception in practice. Institutions for treatment—public and private hospitals—have now specialists attached, a large number of such specialists being considered an evidence of progress and enlightenment. The highest ideal in the examination of a patient is to have him

examined by several specialists, so that institutions pride themselves on the advantages they can thus offer, while in private the wealthy patient is submitted to as many specialists as the doctor into whose hands he commits himself deems necessary. As there is no limit to the number, it is left to the doctor how many days the patient may spend being examined by different specialists, though the patient's financial resources may impose a limit. There is much that seems reasonable to the superficial observer in the development of medicine on these lines. A man who gives years of his life to the study of a limited subject and who sees a great many instances, is bound to have a wide knowledge of his subject. A man who employs some form of treatment on a great number of people is bound to have a wide experience of its effects. A surgeon who is frequently operating for certain kinds of diseases is bound to acquire a manipulative dexterity that enables him to get better results than one who operates on but a few cases.

The Surgeon as Specialist.

The strength and weakness of specialism is seen in the position of the surgeon. The improvement of surgical technique enables the surgeon to open the body and perform drastic operations with little danger to the life of the individual. His results are often so striking in their success that a somewhat exaggerated notion has arisen as to the value of his contribution to the progress of medicine. It has not been sufficiently realised that his interference is at a stage of disease when it has advanced so far as to have damaged tissue and grossly perverted function. He might be said to flourish on the failure of the physician—in the sense that the early and curable stages have been overlooked or unsuccessfully combated. In the vast majority of cases

his operations are not cures, but the removal of the effects of disease by mutilation of the organ, and they often deal but with the more prominent causes of distress, the remote and provoking cause being undetected, as, for instance, in the operative treatment of gastric ulcer or appendicitis.

The reading of surgical books is, as a rule, disappointing to the student of medicine. The surgeon's magnificent opportunities are rarely used for the investigation of disease, and his contributions deal mainly with detailed descriptions of methods.

Under this specialism the possibilities of the surgeon's assisting in advancing our knowledge of symptoms are diminishing. At one time the surgeon had to rely on the exercise of his own powers of perception in detecting the signs of disease, and so his senses were trained and educated, and he was forced to co-relate his findings at an operation with the symptoms he detected beforehand. Now the tendency is towards getting one or more specialists to provide him with a diagnosis, and he operates at their bidding. There is even being evolved another specialist for this purpose, called a "diagnostician"—one who employs a great number of special methods, mostly of laboratory origin, to detect the signs of disease. At the present stage of medical development he is, no doubt, a very useful individual.

The drawback to Specialism.

The advocates of this extreme specialisation can point to a great many successes, and each individual specialist can give any number of instances illustrating the success that has attended the application of his particular method. Admitting all that, and recognising that there will always be some use for each par-

ticular method, the failures remain glaring. Now in medicine, if there should be one thing more stimulating than any other, it is the failures. Success may be accidental and misleading; failures imply a lack of knowledge, and are an urgent call for enlightenment, and for a recognition of fundamental principles.

What then is the conception that would simplify medicine and make all its phases comprehensible to a man of ordinary intelligence? The answer is difficult, for the subject is clouded over with innumerable details, beliefs, and even superstitions. In later chapters I evolve a scheme which seems to be a necessary step towards answering the question, but beyond the fact that I know that a much clearer conception of disease is bound to be evolved, I have not got far enough to see exactly how that conception may be carried into effect in all instances. I therefore content myself with demonstrating how medicine can be simplified in some instances. I am convinced that the conception of specialism dominant to-day is a wrong one, and that helpful as this specialism has been in many ways, nevertheless, instead of enlightening, it tends to darken understanding in a cloud of detail. That each new phenomenon discovered by methods simple or recondite, is of importance in adding to our knowledge of disease, is accepted as a truism, and on that ground specialism seems justified. But there are other matters in the recognition of a phenomenon beyond its discovery which are essential to the progress of medicine.

Where Specialism fails.

It should be recognised that the specialist begins his contribution to medicine after the disease has damaged the body and provided a physical sign. The question arises, how was the physical sign produced? This simple matter is of the essence of medicine,

particularly in dealing with the early stages of disease; and disease will never be properly understood till this knowledge is acquired. The specialist's attitude towards disease is not helpful towards this end but the reverse. His methods are no doubt of great value in detecting disease after it has advanced so far as to damage the tissues, but the aim of medicine being to prevent and cure disease, he does not help to find out the disease before it has produced damage. It is this extraordinary limitation of outlook which retards the progress of medicine. The revelations of the specialist are often so remarkable that the glamour of his successes blinds him and the public to their true significance.

If the conception of medicine that inspires this specialism were true, then the outlook of medicine would indeed be a sad one. It is evident, for instance, that the vast bulk of suffering humanity could never get adequate attention. There are only a few people relatively who could obtain treatment at the institutions provided with a sufficient number of specialists, and it would only be a rich man who could afford the fees of the many specialists who are necessary for his examination, while the operating specialist of great experience could only attend to a relatively small number—and these would be the individuals who could afford his large fees.

The Fallacy in Specialism.

However plausible and reasonable the conception may appear, it is fortunately based upon a fallacy. What that fallacy is it is difficult to see.

The onset of disease in the human organism modifies the tissues and functions of the different organs of the body. At present the specialist in directing his attention to the study of the effect of disease on some

particular organ is oblivious of the original hostile force, and the effect of it upon other organs.

The matter may be presented in another way. The manifestations of nature are infinite in their variety and, to the untutored mind, seemingly chaotic, but the fundamental forces producing them are few. A recognition of these forces at once brings the chaos into an orderly arrangement, and permits of an easy comprehension of the significance of the manifestations. In other words, the more thorough the knowledge of the laws of nature, the easier it is to understand nature's manifestations. An increase in knowledge of natural laws leads to simplification. Disease is governed by natural laws; its manifestations are infinite in variety and are to-day chaotic and difficult to understand. What is now called progress is but the recognition of an additional number of these manifestations, and an ever-increasing difficulty in comprehending their significance. From this way of looking at the matter the suspicion should be aroused that what is considered to be progress in medicine cannot be true progress because it does not make for simplification.

It has not been sufficiently realised that the majority of specialists deal with disease at an advanced stage, so that among teachers in a school of medicine or in a hospital with many specialists, none of them, singly or combined, recognise the early stages of the disease. When a patient's condition is complex in the sense that his complaint involves several organs, the defect of specialism becomes evident. I could illustrate this aspect in many ways, but no illustration reveals the defect of specialism as practised to-day, so well as the attitude towards the pregnant woman who suffers from heart disease. By bitter experience general practitioners have recognised that in certain diseased states of the heart there is a great danger to the life of the

woman and child. For over 50 years there have been many attempts to find out where the danger lies, and to recognise the signs that foretell disaster and the signs which need cause no anxiety. Yet the most recent of our text-books show no advance on the views of 50 years ago.

There is a physiological problem involved, but no physiologist has attempted its solution. There are problems intimately connected with clinical medicine, yet physicians have no opportunity for studying the pregnant woman. The obstetrician sees her, but no obstetrician has yet learnt the elements of cardiac symptomatology sufficiently to enable him to acquire the necessary information. Here is a problem of the very first importance, which will, time and again, confront every general practitioner, and in the whole hierarchy of a medical school there is not one teacher, or even a group of teachers, capable of acquiring, far less of imparting, the necessary knowledge.

I recognise the great addition made to the knowledge of medicine by men who have devoted themselves to special branches. It is manifest that in limiting themselves to one section of medicine, however profound their inquiries may be, they will nevertheless obtain only a partial view of their subject. For instance, a bacteriologist who does not see the effects of microbic invasion on the human subject cannot have a thorough knowledge of the disease which a microbe can produce. If he would understand his subject fully, he must know the symptoms which are produced by the various organs of the body.

I trust to show in the recital of my experiences that I have been able to add to our knowledge of the diseases of the heart. The most important of these observations were detected by studying the effects of heart failure on other organs. Had I restricted myself

to the study of heart symptoms, I would not have been able to detect the evidences of early heart failure. Moreover, a large number of the heart affections are secondary to disease of other organs, so that to understand the nature of many affections of the heart it is necessary to have a knowledge of other organs.

CHAPTER IV

RESEARCH IN MEDICINE

MEDICINE has benefited greatly by the progress that has been made in allied subjects. I have already referred to the great help rendered by the pathologist; benefits have also accrued to medicine through the experimental researches of the physiologist. Although most of the researches are made without any reference to their application in practice—what may be called academic physiology—the results of these experiments have been of the greatest assistance to the worker in every field concerned with the investigation of disease. Other laboratory workers—the pharmacologist, the bacteriologist, the physiological chemist, have contributed much that has been of the greatest service in the practice of medicine.

There has been of late years a good deal of participation in clinical work by laboratory workers, such as physiologists and pharmacologists, and their influence has been all to the good. Personally, I have derived very great assistance in attempting to solve clinical problems, from the observations of what one might call academic physiological researches as well as from physiological researches performed with a definite clinical object. While we cannot but welcome the realisation by both physiologist and clinician of the need of a partnership, and while we recognise the importance of this development, it is in danger of being rendered

ineffective unless a clear outlook is obtained of what kind of knowledge is necessary for the prosecution of research in disease. The joint investigation by physiologist and clinician has been concerned with the advanced stages of disease, and the restriction of investigators to this aspect will not carry us far in achieving the main object of research.

Common sense and worldly experience tell us that if we would overthrow an enemy we must understand all his resources; if we would overcome a difficulty, we must understand where the difficulty resists our attempts; if we would solve a problem, the nature of the problem must be understood. The principle underlying these statements should be applied in the prevention and cure of disease, for it is manifest that the more thoroughly we know the disease, the conditions that favour its onset, the signs of its invasion, and of its progress, and the circumstances that favour or retard its progress, the better equipped we will be for preventing its recurrence, or curing it before it has damaged the tissues.

In applying this principle to medical research it is essential that the investigator should himself discover the problem to be solved, and should have carefully studied all the features of the problem before he takes special steps for its solution. As disease can be recognised in the living only by the symptoms it produces, a knowledge of symptoms should be an essential qualification for the investigator to possess. The study of symptoms has been pursued ever since medicine was an object of investigation and the study has been carried so far that experienced physicians have stated that the limit of our knowledge of symptoms revealed by the unaided senses has been reached, and any further advance must be made by the invention of new methods. They turn expectantly to the laboratory

worker, and he has risen to the occasion and given a great many different methods for the bringing to light new manifestations of disease, as can be gathered from the description of methods in pages 35 to 39. Notwithstanding the enormous development of methods for the detection of symptoms it is difficult to explain without copious illustrations the view that I have arrived at from long experience, namely, that there is an aspect of symptomatology which has been missed but which is essential to research as well as to practice. I shall deal fully later with the subject; here I wish to direct attention to certain results of investigation that have been already obtained by putting into practice the principle of symptomatology to be explained.

In the recital of my personal experience I explain the steps taken to find out the action of digitalis. When the effect of digitalis was carefully observed on different people it was found that hearts reacted in a variety of ways. In some digitalis caused a decrease in the rate, in others the same doses had no effect on the rate. In some the heart became irregular under the action of digitalis; in others the rhythm was unaltered. In cases where there was one kind of abnormal rhythm digitalis reacted in one way, and in a different way with other abnormal rhythms.

It was found that before the action of digitalis on the human heart could be understood it was necessary to have a knowledge of the different forms of irregular heart action. When this knowledge was acquired, then the investigation into the action of digitalis became possible, and the result enables us now to use this remedy with a certainty that had never before been attained. Indeed, until this research was undertaken, the mode of action of digitalis, and the condition which indicated its use were not recognised. It was known that in some cases digitalis did good, but there was no

clear recognition of the kind of case, and digitalis was administered in a rule of thumb fashion, so that every kind of heart trouble was supposed to be an indication for the use of this drug.

This investigation brought to light a fundamental principle which has been practically ignored in the investigation into the action of remedies, that *the action of the drug was modified by the nature of the disease.*

The lesson to be derived from this experience is that before an investigation can be made into the action of any remedy on the human subject it is necessary the investigator should possess a knowledge of the symptoms of the disease with which the patient is afflicted. Though this statement is so self-evident that it needs no elaboration, it has nevertheless been systematically ignored. Probably no other drug has been the subject of more investigation, both by physicians and physiologists and pharmacologists. We can now see that it was hopeless to expect from physicians an accurate description of the effect of the drug while they were ignorant of the symptoms, and it was equally hopeless to expect that experiments on the healthy hearts of animals would reveal the effect of digitalis on the diseased heart of man.

The principle that a knowledge of symptoms is essential to an inquiry into the effects of remedies has never been recognised, and its neglect is the reason that so much investigation is rendered sterile. The history of the investigation into the source of danger in chloroform anæsthesia is a case in point. Innumerable inquiries, mostly fruitless—so far as their bearing upon its practical use—have been made into this subject, because it has never been recognised that the danger of chloroform administration can never be detected by the study of its effect on healthy animals, nor can any anæsthetist discover its effects upon the human heart

until he has made himself familiar with the symptoms of disease, and the manifestations of a disordered heart and nervous system. As chloroform affects the nervous system and the heart, the disorders of these systems must be known. It is undoubtedly due to the neglect of this principle that the action of drugs and other remedies is in such a confused state and our pharmacopœa is mainly a storehouse of the beliefs of bygone ages.

My object in alluding to this part of the subject here is to show that there is needed a new concept in the investigation of disease. The discoveries of the physiologist and of investigators in a limited field of medicine, as bacteriology, have led to the notion that they are also capable of investigating the wider fields of disease. The barrenness of a great deal of investigation can be traced to the neglect of the principle, that a knowledge of symptoms is necessary for investigation into all the problems connected with disease.

The training necessary for an investigator must include the acquisition of this knowledge. It is hopeless to expect a man ignorant of symptoms to investigate disease or direct others in its investigation. Here and there a striking discovery may be brought to light by workers in some limited field, but such investigations are haphazard and the discoveries are accidental, whereas the prosecution of research should be systematic and conducted by people with an experience of the symptoms of the disease to be investigated. If the need of this knowledge is appreciated, then it will be seen how futile it is to attempt to train an investigator by sending him to laboratories, where he is effectively shut off from the opportunity of acquiring the knowledge necessary to attain his object. This can be recognised if we glance at the results of research scholarships. The scholar is invariably sent to work in some

place, as a laboratory, where he cannot get a personal contact with disease, whereas, if a proper view was taken of the subject, the first step in his training should be to acquire a knowledge of the diseases he intends to investigate—a knowledge that can only be acquired by personal contact with individuals suffering from disease.

The Bacteriologist and Medical Research.

The neglect of the principle essential to research, that an investigator must be familiar with a problem before he undertakes research, can be illustrated by the limitations of bacteriological research. The discoveries of the bacteriologist are continually being quoted as evidence of the progress of medicine, but the glamour surrounding them misleads as to their significance. It is scarcely realised how lacking in system and how haphazard is this method of research in medicine. Even the greatest discoveries have been accidental. Vaccination was discovered by an observing general practitioner having his attention called to the fact that dairy maids did not suffer severely from smallpox, while the microbic origin of disease came through a chemist (Pasteur) studying the process of fermentation. It is true that Lister's discovery came through a systematic research with a definite object in view, but he followed the principle referred to and made himself familiar with all the aspects of the problem. Once it was recognised that many diseases were of microbic origin, it followed naturally that the systematic search for microbes in specific diseases would be attended by a certain measure of success. The success that has attended the research in certain tropical diseases and diseases allied to them (as trench fever) in identifying the cause of the disease and the conditions that favour its occurrence, cannot be too highly appreciated. But when we come to con-

sider the ordinary diseases that afflict the community as shown in Figures 1 and 2 we should realise that recent discoveries do not carry us far in dealing with such diseases.

In medicine it takes many years before the value of any discovery can be estimated; we know with what enthusiasm each new discovery is hailed—a new era in medicine is often foretold, and after a few years the limited significance of the discovery is gradually realised. There is no doubt that infections play an enormous part in the production of disease, and that being so, laboratory methods are essential in their detection. Important as the field of bacteriology is, it must be recognised that there is a vast deal more in the study of disease than the mere detection of the pathogenic organism. It must strike everyone who has had the opportunity of studying disease in the human being, that there is a great diversity in the response of individuals to microbic invasion. What is called the virulence of the disease is so variable that the factors responsible must be recognised if our knowledge of disease is to be so complete as to enable us successfully to combat it. It is manifest that in addition to peculiarities of the microbe, the soil has a distinct influence. If it be true, as is generally assumed, that most people at one time or another suffer from a tubercular infection, it follows that the vast bulk of the community have the power of successfully combating it. Men who fall victims to such a disease as consumption must therefore have some other factor which favours the growth of the tubercle bacillus and prevents recovery.

The improved methods, and the skilful technique of many able investigators have brought to light the organisms that are the active agents in producing a great many diseases, many of them being those diseases which are common in the community, and which do so

much to lower the health of the people. The rule that one discovery is but a step in the solution, and not the solution of the matter, applies with much force to bacteriological investigation. There is always a tendency to look upon the discovery of one agent, though that agent be the chief or essential factor in causing an infective disease, as solving the problem of the origin or nature of that disease. Experience shows invariably that is but one factor, and that to stop there or to begin a line of treatment, preventive or curative, based on such limited knowledge will almost certainly lead to failure, and even to the discrediting of the significance of the original discovery. If then a discovery of the first importance, even of an essential agent in the production of diseases, be made, it should always be recognised that it is only one step forward, and that the journey is far from being completed. It will often happen that the further steps in the discovery cannot be taken by the investigator who has found the essential factor, but the pursuit must be taken up by others who employ different methods and who have more favourable opportunities.

The symptoms provoked by the entrance of a pathogenic germ into the body are to a great extent unrecognised. On this account bacteriology is hampered, and much of its work is rendered valueless and its opportunities for benefiting suffering humanity limited. While the laboratory studies may reveal the microbe producing a disease, and the peculiarities of the microbe may be studied with particular care, in its behaviour in nutrient media, and in animals, there is an urgent need to recognise the symptoms it produces on its entrance into the human body. It is manifest that it is only an infinitesimal number of people who can ever have a bacteriological examination. If then, bacteriology is ever to give its full benefit to

humanity, it is incumbent that other means must be taken to render its discoveries of use. Later I point out as a necessary step in the advance of medicine, that as soon as a discovery is made by some laboratory method, an endeavour should be made to supersede the method by some simpler one that can be employed by the general practitioner. In applying this rule to bacteriology, the means by which this should be done is to recognise the phenomena produced by the entrance of the microbe into the human body. It is therefore imperative that if bacteriological research is ever to bear fruit that the symptoms produced by each microbe should be recognised by the doctor who sees the patient.

This simple statement is so evident that it would seem unnecessary to insist upon it, yet in practice it is ignored. It is only a few infectious diseases that are recognised by their clinical evidences, and then because of certain outstanding phenomena, such as a rash in measles, scarlet fever, smallpox, or the exudation on the tonsils in diphtheria. The vast majority of infections doubtless produce signs as distinctive—only we are not yet sufficiently educated to recognise them, and it is shutting our eyes to the light to say that because we do not recognise them they do not exist.

The importance of this attitude to bacteriological research is emphasised where attempts are made to apply the result of it in the treatment of disease. In certain diseases the employment of vaccine therapy has been so striking that great expectations have been raised as to its universal application in all sorts of disease. This has led to its indiscriminate employment, so often with absolutely no good result that a reaction is bound to set in, its usefulness denied, and its claims derided. It would not be describing the position to-day unfairly to say that vaccine therapy is in a state of utter confusion. The evidence produced by

its supporters is in many cases so lacking in clinical details that no reader can grasp clearly the condition in which the treatment is effective. The result is that a great many cases are submitted blindly to a course of treatment for which they are totally unsuited, and the failures then cast a reflection on the treatment. The bacteriologist complains, quite justifiably, that he is called to treat cases, manifestly ill from some microbic invasion, after the physicians or surgeons have exhausted their means of treatment, and his failure to effect a cure is supposed to discredit his methods. On the other hand, when bacteriologists themselves seek to employ their methods, their lack of clinical knowledge renders their attempts at treatment very often of no avail. Not only are they unable to recognise the symptoms produced in man by the invasion of a microbe, but even when they have obtained distinct evidence of phenomena produced by microbic invasion they are not competent to say whether the presence of the microbe is incidental, the primary cause, or secondary to some other condition. A simple illustration will bring this out clearly. A man suffered from a pustular eruption and consulted a bacteriologist, who found a microbe in the pus. This he cultivated, and from it made a vaccine, with which he treated the patient for several months. The patient, being no better, consulted another doctor, who recognised that the man suffered from scabies, and by appropriate treatment of the scabies infection cured the patient in a few weeks. This is not an uncommon incident, either with scabies or other conditions, where the microbic invasion is secondary. In fact, a large number of diseases are secondary, and the lack of a knowledge of this fact renders ineffective and brings into disrepute the employment of such means of treatment as vaccine therapy.

PART II.

PERSONAL EXPERIENCES

INTRODUCTION

THE view that a knowledge of symptoms is necessary for all investigations into disease has been so little comprehended that insufficient consideration has been given to their study. It has been assumed that the knowledge has been as thoroughly acquired as circumstances permitted. A careful consideration reveals the fact that the knowledge is not only incomplete, but that the necessity for such a knowledge and the methods necessary to acquire it have never been understood. If symptoms are ever to obtain their proper place in research work, a great deal of spade work will require to be done in order that their specific value may be determined.

As the steps I have taken to acquire this knowledge are different from those usually pursued, I shall, before dealing with the subject of symptoms in detail, give a description of those steps, and occasionally illustrate how the application of certain principles guided me in the discovery of unsuspected morbid states. Although a great part of the investigation covered a large field of medicine, more particular attention was directed towards affections of the circulatory system; and as I have carried the study further here than with other systems, most of my illustrations will have re-

ference to affections of the heart. I have realised, however, that the principles suited for an investigation of the diseases of one organ are applicable to those of other organs, though the methods of applying the principles are different. Moreover the mechanism by which many different symptoms are produced are fundamentally the same, however distinct the organs themselves may be in structure and function.

I do not claim to have made any discovery of moment. Startling discoveries come only to the few, and are mainly accidental. All I claim is that I have attempted to render research systematic, by acquiring a more thorough knowledge of disease, and so perhaps have prepared the way for a scientific attempt as against the somewhat haphazard methods current today.

I am quite conscious that my own researches do not carry us far, and reveal only a very little that is new, and I do not cite them for the purpose of claiming any credit for what they have discovered. My object is rather to show others what might be done if this line of investigation was systematically pursued. I myself realised the significance of this line too late in life for me to develop it fully, and when the opportunities for its proper prosecution had been lost. If young men on their entrance into practice will catch the spirit of such an experience, there lies before them an excellent opportunity for placing research in medicine on a sure foundation.

CHAPTER I

THE OPPORTUNITIES OF THE GENERAL PRACTITIONER

The Shortcomings of a general practitioner's medical education.

It is forty years since I qualified. After a year in hospital as house physician, I entered general practice in an industrial town of about 100,000 inhabitants. I started my work fairly confident that my teaching and hospital experience had amply furnished me with a competent knowledge for the pursuit of my profession. Like all other graduates, then as now, I was under the impression that a knowledge of the physical signs of disease was the most important qualification for the kind of work I had to follow, and I had been fairly efficiently trained in this respect. I was not long engaged in my new sphere when I realised that I was unable to recognise the ailments in the great majority of my patients. They presented aspects of disease with which I was unacquainted. In my student days in the hospital wards and even in the out-patient department, then as now, our examinations were in the main directed to finding out gross physical signs. In general practice I found that most of the patients had no physical signs, and often such physical signs as I detected had no seeming relation to their complaints. The patients felt ill or suffered from pain or other disagreeable sensation. These subjective symptoms had received but scant attention in our training and

I was unable to appreciate them. At the time I looked upon this lack of knowledge as personal, never doubting but that experienced physicians had long ago solved what seemed a simple problem, as I then assumed it to be, of the patients' sensations. Although I read the leading text-books assiduously, I could find little help, and concluded that these phenomena which puzzled me were so well known that they did not need particularizing in text-books. Curiously enough this was the explanation given to me in later years when I asked a distinguished physician why in his book he did not describe some simple phenomena.

Attempts to overcome a lack of knowledge of clinical symptoms.

For some years I went blundering on, gradually falling into a routine, i.e., giving some drug that seemed to act favourably upon the patient, till I became dissatisfied with my work, and resolved to try and improve my knowledge by more careful observation. I could not for a long time think of any definite scheme, and I tried first one way and then another. It might seem that with so many problems daily confronting me I should have had little difficulty in formulating a plan for the study of my patients' complaints, and now with my experience I can see that the task can be made quite simple. But until one's eyes are opened so that medical questions are looked at from a new aspect, it is impossible to know how to set about investigating them, as is indeed evident from the fact that, even at this day with the great cry for research, few clinicians know how the matter should be pursued. After trying one way after another I put to myself the question, "Do I understand the meaning of one single sign or symptom in all its aspects, for example, the

mechanism of its production, or the bearing of its cause upon the patient's future health?" In this way I was brought to a realisation of the fact that I did not understand the full meaning and significance of a single symptom. It was about 1885, I think, that this idea occurred to me, and at the time I had no thought of undertaking what may be called research. I thought then that the great physicians had already solved all the problems essential to the recognition of the signs of disease, and my undertaking was simply for the sake of personal instruction. It was not until many years later that I recognised the significance of this step, not indeed until after it had revealed to me many facts essential to the study of disease which had never received proper attention. For that reason my notes of cases are not so full as to enable me to give convincing proof to others of the result of my search, although I had stored many of the facts in my mind and tested them in my practice.

Methods adopted to investigate the significance of symptoms.

In noting each symptom which my patients presented, I soon collected such a number, of whose full significance I was to a great extent ignorant, that it became clear to me that, if I was to make any progress, I must restrict myself in the first instance to only a limited number of symptoms.

The plan I pursued was to note carefully the symptoms of which the patient complained and to seek in the patient for any sign, however trivial, whose meaning I did not understand. By following this plan I soon detected plenty of signs which I had hitherto overlooked, such as the facial aspect and expression, the colour of the cheeks and lips, the movements in the vessels in the neck, variations in the character and in the rhythm of the pulse, the appearance of goose flesh,

the difference in heart rate with variations of temperatures and so forth. It was impossible for me to investigate all the phenomena, and I made only very slight progress in most, while a few led me into a search that has not yet been completed.

The patient's sensations; their importance, and how they should be investigated.

But my attention was arrested by the story the patient had to tell, and as years passed and I became more and more versed in the study of symptoms, the recognition of the patient's sensations gave me a deeper insight into the nature of his complaints than I had thought possible. If one looks at the diseases depicted in Fig. 1, which represents roughly the ratio of disease from among 1,000 consecutive cases, it will be seen that the bulk of the complaints are made manifest mainly by the sensations experienced by the patient.

It is assumed that the nature of a patient's complaint as represented by the description of his sensation is one of the simplest problems of medicine. In books specially devoted to diagnosis little attention is given to this problem because it is assumed that any man with reasonable intelligence can readily get all the useful information by a few common-sense questions. The fact that the matter is dealt with perfunctorily in text-books and in the routine teaching seems to stamp this idea on the student's mind.

Moreover it is not only assumed that the matter is easy to investigate, it is also taken for granted that the information is usually of little value when obtained partly because other means of inquiry are more precise and other signs can be more readily demonstrated, and partly because the patient's description is coloured by his own individuality, and thus either exaggerated or rendered too vague and indefinite to be of use.

When all this is recognised there is still a great field of useful information to be acquired. In fact, from the patient's statement can be acquired information that is absolutely essential to the recognition of disease, especially in the early stages. It would not be exaggerating to say that the failure of medicine to detect disease in its early stage is due to the fact that the sensations of patients have never been adequately investigated. Even when I had recognised the importance of this mode of investigation I found the greatest difficulty in eliciting the sensations, and in understanding the mechanism of their production. The knowledge I have acquired of the methods, and the information I have thereby gained, show that there is in the patient's sensations a field of enormous value, but it requires a long and tedious training before the physician becomes capable of fully utilising this method of examination.

CHAPTER II

THE INVESTIGATION OF PAIN

In order that the methods for the investigation of the patient's sensations may be understood, I give here the plan I pursued to elicit the information which the sensation of pain was capable of furnishing.

When I had set myself specifically to investigate pain, I found from time to time as the inquiry proceeded that I had omitted points that were necessary to its elucidation, and had to go over again and again the same field, fortified by the additional knowledge I had acquired. I detected repeatedly in my search a lack of precision that vitiated many of my earlier observations. For instance, if a patient had manifestly an affection of the stomach and had vomited blood, and suffered from pain which was felt in the "pit of the stomach" or in the neighbourhood of the epgastrium, I would note the observation at first as a "pain in the stomach." In watching a woman in labour I would put the pain down as being felt in the uterus. If I pressed over the stomach or liver and the patient winced, I would note the observation as a "tender stomach" and a "tender liver." As the inquiry proceeded, I found certain other facts which made it doubtful if these statements were correct, so that I had to start again and make notes simply stating the region in which the pain and tenderness was felt, without reference to the organs that lay in that neighbourhood. But here again, I was often met with a difficulty, namely, that the patient's recollection of the pain was often so vague that the situation was, not

infrequently, misleading. I therefore requested the patient carefully to note the position of the pain in future attacks, and I seized the opportunity which a general practitioner often has of observing the patient while in an attack of pain and then I carefully noted all the circumstances associated with the pain. In this way I found that many pains started in one place and shifted to other regions, and by noting accurately the radiation another source of information was opened up, which ultimately became of the greatest help in recognising the particular organ that was affected. The peculiar radiation of pain shows that there are paths in the spinal cord which are so far unrecognised. A recognition of the nature and meaning of this radiation revealed that the usual idea, that pain is often so diffuse and vague as to be of little value as a symptom, was wrong, for the production of pain is due to a stimulation of certain cells in the central nervous system, and the location of the pain is in a field definite and distinct in the peripheral distribution of nerves from these cells. These particulars I had to train myself to observe accurately, and in doing so I discovered that there were frequently other disturbances, and I began to realise the importance of associated signs.

Importance of Ross's description of the nature of pain.

In this way I collected many details, but they were rather confused and lacking in any principle which would indicate a co-relation with other signs that were frequently present. At this time Dr. James Ross published his paper on referred pain (see *Brain*, Vol. X., 1887-88, p. 333), and it at once gave a clue which led to a clearer perception of the subject. I do not recollect any article which was, to me, of so much help. It came as a

revelation and it afforded me a guide which I found to be of the greatest use. In that paper Ross described visceral pain as being of two kinds, one a pain felt in the organ, and the other a pain resulting from a stimulation of the central nervous system, the pain being referred to some portion of the external layers of the body wall. The first pain he called "splanchnic," and the second—the referred pain, he called "somatic." He illustrated this division by taking a case of gastric ulcer, the "splanchnic" pain was felt in the organ in the epigastric region, the "somatic" pain was referred between the shoulders, and he showed that the cutaneous nerves in this latter region left the spinal cord at the same level as the sympathetic nerves which supplied the stomach.

The necessity of not mixing observations of facts with theories as to their cause.

In my inquiries I found it necessary, as I have said, in making an observation, not to go beyond the actual facts. Thus, if a pain was felt in any region, such as the epigastric, or under the right ribs, I noted that fact, but did not say the pain was in the stomach or in the liver, even though these organs were manifestly diseased.

When I read Ross's paper, I was not satisfied as to his splanchnic pain. If the pain was felt at a distance from the organ and the organ was manifestly the cause of the pain, then the explanation that the pain was referred or somatic was reasonable. But because a pain happened to be felt in the immediate neighbourhood of an organ, it was not sufficient reason for assuming that the pain was actually felt in the organ. I discussed the matter with Ross, and he maintained his view and mentioned a case of gastric ulcer he had then under his care, in whom the pain in the epigastrium

was severe and limited to so small an area, that he felt certain if he pushed a long needle through this area he would penetrate the ulcer. I pointed out that if the patient took a deep breath, the stomach and the ulcer would be shifted, but would the site of pain shift? Putting this to the test, I found the pain did not shift.

Is pain felt in the tissues of an organ?

Here then was a problem to solve: Was there such a thing as pain felt actually in a viscus? It was a view so universally accepted in those days, and even to-day, that its discussion seemed uncalled for. Yet when I looked for strict evidence I could find none. Hitherto, such evidence as existed was drawn from the fact that the pain was situated in some part of the body in which the offending organ was supposed to be. If, by pressing over an organ through the tissues of the abdominal wall pain was elicited, this was unhesitatingly described as a "tender gall-bladder," a "tender appendix," a "tender spleen," according to the region that was tender.

The recognition of cutaneous hyperalgesia as a result of visceral stimulation.

It was some time before I obtained another kind of proof which threw a flood of light on the whole matter. I had been attending a man with severe gall-stone colic, and after a few days in bed the attacks had subsided. On his first visit to my consulting room, after he got up, I observed that he held his right arm stiffly away from his side. I asked him why he carried himself in this fashion, and he replied that his skin on the right side was so tender, he could not bear his clothes to touch it. I asked him to strip and found an extensive area of hyperalgesia over the right side in the region shaded in Figure 3. In addition to the hyperalgesia,

he had to breathe quietly, as when he drew a deep breath, a pain seized him as it does in pleurisy (as a matter of fact after a similar attack of biliary colic some years later a colleague diagnosed a pleurisy be-

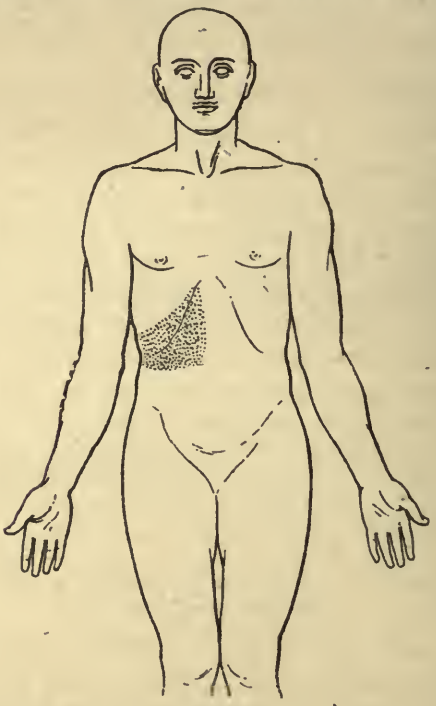


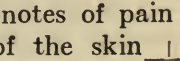
FIG. 8.

The shaded area shows the region in which cutaneous hyperalgesia occurred after an attack of gall-stone colic, in the first case in which a definite area was detected due to visceral disease.

cause of the tenderness of the external body wall and of the pain in breathing). This case led me to consider whether hyperalgesia of the skin did not appear in other diseases, and I soon found that it was far from an infrequent sign. In the examination of many people with diverse diseases, I found that not infrequently

the site of the pain coincided with an area of cutaneous hyperalgesia, and that the pain, arising from pressure exercised over the diseased organ, was produced by stimulating the over-sensitive skin. This suggested that even Ross's splanchnic pain was a referred pain and arose in the same way as the hyperalgesic skin.

The relation of skin areas to visceral organs.

It had been my habit in making notes of pain and other signs, such as hyperalgesia of the skin of the abdomen, to make a simple diagram—  representing the conventional division of the abdominal surface into nine areas. I noted the site of pain, etc., by a pen scratch. On looking over a large number of cases of gastric ulcer, I was struck with the frequency of these signs in the epigastric region and in the middle line. This suggested the possibility, that the site of the ulcer had a distinct relation to certain areas of skin, and that the situation of the ulcer might be recognised by the more accurate perception of the site of pain and hyperalgesia. It was a long time before I obtained sufficient evidence to demonstrate this relationship. I have now gathered notes of a sufficient number of cases practically to establish my view. These notes record and compare the signs met with during life with the site of the ulcer found on operation or after death. In this way the pain in gastric ulcer was found to vary with the site of the ulcer, the higher in the epigastrium, the nearer would the ulcer be to the cardiac end of the stomach—the pain of an ulcer at or near the duodenum being situated at the lowest part of the epigastrium. I also made observations on the pain of intestinal obstruction. Advantage was taken of the fact that in obstruction of the bowel the peristaltic wave which produces the pain does not pass beyond the point of obstruction, so that in watching

cases of obstruction, as strangulated hernias, I was able to note the onset of a peristaltic wave by the pain, and the patient's account of the radiation of the pain, till it passed off. In this way I obtained a knowledge of the region in which the pain started, and to what place it shifted, and it was found always to stop at one place which I inferred would be due to peristalsis stopping at the part obstructed. These facts I co-related with the portion of the bowel which was obstructed as revealed at operation or post mortem. On watching cases suffering from diarrhœa, the manner in which the pain began and proceeded till the expulsion of the contents of the

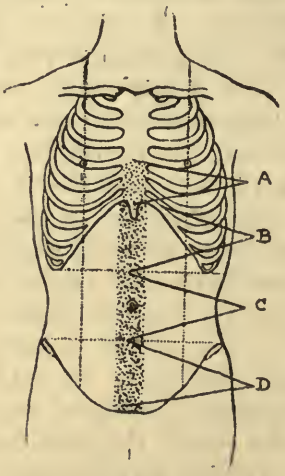


FIG. 4.

The shaded part shows the area in which pain is felt in certain affections of the digestive tube. Peristalsis passing along the intestinal tube may give rise to pain felt descending in the shaded areas.
A. Area in which pain is felt in affections of the Æsophagus.
B. Area in which pain is felt in affections of the Stomach.
C. Area in which pain is felt in affections of the Small Intestine.
D. Area in which pain is felt in affections of the Large Intestine.

bowels gave additional information of a valuable kind. In one case, a patient with heart failure from auricular fibrillation, pain, followed by diarrhœa, was always set

up when digitalis was given. The pain in this case would start in the epigastric region, slowly descend down the middle of the abdomen till it reached just above the pubis. The call to defecate then became urgent; on voiding a loose motion, relief was obtained. I was able then to construct a diagram, such as Fig. 4, which indicates the regions in which pain is felt in painful peristalsis of different portions of the bowel. This diagram I have tested for many years and find it is of considerable value.

Different kinds of hyperalgesia.

The accurate perception of the part in which the pain starts and the parts to which it spreads is greatly assisted when tenderness of the tissues of the external wall (skin and muscles) is present. A prolonged inquiry has revealed that the hyperalgesia is of different kinds. There is a superficial cutaneous hyperalgesia detectable by scratching lightly with the head of a pin, and a deeper cutaneous hyperalgesia elicited by lightly pinching the skin between the thumb and forefinger. The superficial hyperalgesia is often absent but when present the deeper is also present and extends over a much wider area than the superficial.

Recognition of muscular hyperalgesia and contraction as a result of visceral stimulation.

There was also hyperalgesia of deeper structures, the most important being that of the muscle especially shown in portions of those of the abdominal wall. This deep tenderness was usually associated with a varying degree of tonic contraction of these muscles. The extent of this muscular hyperalgesia and contraction is not easy to make out in many cases, but with practice and careful perception, a certain facility is acquired by which information of an instructive kind can be ob-

tained. The muscular contraction is not infrequently overlooked, or, when perceived, is mistaken for a tumour in the abdominal cavity. This is due to the fact that there is a peculiar reaction of the flat muscles of the abdominal wall to visceral stimulation, in the sense that often the whole muscle does not contract, but only limited portions, and these hard and firmly contracted portions of muscle have a distinct resemblance to a tumour in the abdominal cavity. I have repeatedly seen cases where distinguished physicians and surgeons have diagnosed a neoplasm, an enlarged pancreas, a thickened pylorus, an enlarged appendix, but when the abdomen was opened none of these conditions was found, nor was anything found in the abdominal cavity to account for the object felt before the abdomen was opened. I have satisfied myself frequently that it was this muscular contraction that was felt.

The viscero-sensory and viscero-motor reflex.

It will thus be seen that there are two distinct reflexes found in the external body wall as a result of visceral disease, which I have suggested should be distinguished by the term *viscero-sensory reflex* and *viscero-motor reflex*. The former being shown by pain and hyperalgesia, and the latter by muscular contraction, the result of a stimulus arising in a viscus and passing to the sensory and motor centres in the spinal cord.

I have come to be very suspicious of the expression "tender spleen" or "tender gall bladder," for the reason that the pain elicited by pressure so readily provokes muscular resistance, that these organs are not likely to be palpable when diseased or "tender," and that the tenderness really arises from the hyperalgesic muscle protecting the diseased organs.

The correlation of the sensory and motor reflexes with the conditions provoking them.

I have tried to correlate the pain and these reflexes—sensory and muscular—with the conditions provoking them. I have only been partially successful, as the subject is an extremely difficult one. I attacked the problem in two ways. First I attempted to recognise the organ which produced the reflex phenomena in any given situation. This I was fairly successful in doing, and my results have been confirmed and extended by some surgeons, notably by Mr. Ligatt, who has employed the method for diagnostic purposes in a very large number of cases with gratifying success, though he chiefly concerned himself with the cutaneous hyperalgesia. Second, I tried to find out what the nature of the stimulus was which provoked these reflexes. I tried to find out the tissues which were sensitive to direct stimulation by pinching and pricking with a pin. I found that the various structures of the external body wall reacted in different degrees, the skin of course being most sensitive, and the muscles sensitive in a less degree. In the abdomen I found at times sensitiveness in the loose areolar tissue external to the peritoneum. But when I applied these ordinary stimuli to the visceral surface of serous membranes (peritoneum, pleura, meninges of the brain) I got no response. The same result followed on stimulating, in this way, all the viscera—bowel, kidney, liver, lung, heart, etc. When I looked at the subject broadly, I found that all the tissues that were sensitive to pinching, pricking, etc., were supplied by the sensory cerebro-spinal nerves, and that to all the tissues that were insensitive to such stimulation, no sensory cerebro-spinal nerves had been traced. By means of this method the distribution of the sensory nerves of the spinal cord can probably be mapped out. For instance, by pricking with a pin an

orifice of the body, e.g., the anus, it can be shown that as soon as a certain line is passed at the margin of the skin and mucous membrane, all sensation disappears. Other orifices, such as the mouth, nose and air passages, show a gradual diminution of sensation, but I have not been able to determine the limits. At and below the vocal cords, stimulation only produced a cough and no sensation. Stimulation of the visceral pleura gave rise to cough but to no sensation. The only serous membrane I found sensitive was the visceral layer of the tunica vaginalis. In tapping a hydrocele one can scratch the parietal layer and elicit no sensation, but the slightest scratch on the visceral layer produces exquisite pain.

It is well known that viscera do give rise to pain, though they are insensitive to pricking and pinching. A long series of observations led to the view that pain produced by the viscera is not due to any sensitive structure in the organ itself, but arises from a stimulus being sent into the central nervous system. This stimulus may excite any nerve centre which it reaches, and such nerve centre reacts according to its function —a secretory nerve centre by modification of secretion, a nerve centre supplying a muscle, by contraction of the muscle; a sensory nerve centre, by pain and hyperalgesia in its peripheral distribution.

The nature of the stimulus adequate to produce pain.

The next question which arose was what was the nature of the stimulus which gave rise to these signs? The locality of the pain and hyperalgesia only indicates the offending organ. Was it possible to differentiate the nature of the disease processes, as inflammation, pus formation, modified tissue growth, embarrassed function?

The search for this stimulus was a very difficult one,

and my surgical opportunities were not sufficiently extensive to enable me to carry the matter very far. Early on in these investigations I found enough evidence to satisfy me that one form of stimulus was undoubtedly the cause of certain kinds of pain, some of them of the most agonising kind—namely, contraction of non-striped muscle. A great bulk of my observations were made upon intestinal and uterine contraction, gall-stone, and renal colic and cardiac pain. Every obstetrician recognises the severity with which the pain of uterine contraction during labour is felt in the back over the upper part of the sacrum. This pain is usually attributed to the pressure of the child's head on the cervix. But it occurs in contraction of the uterus after the child has been born, during what are called "after pains." In the same way the pain in the front of the abdomen is felt in the same place whether the uterus is large and full, or empty, as after birth of the child. In rare cases the pain is felt most severely in the upper part of the thighs. It is not necessary to quote the whole series of observations of the pain observed in diarrhœa and obstruction of the bowels, or in renal and gall-stone colic. All these have features in common, differing only in site and radiation and associated phenomena. The pain is of a character which, when recognised, at once indicates that it arises in a hollow muscular organ. The relation of the pain to contraction of non-striped muscle was proved, to my mind, by a crucial test. I had occasion to resect a piece of small intestine in a man who was under no anæsthetic. While the abdominal organs were exposed I tested their sensibility, and cut and stitched the bowel, and no sensation was produced. I had prepared the upper portion of the bowel and wrapped it in a warm sterilised cloth and laid it on one side. I noted that the patient occasionally

groaned as if in pain, and I asked him if he suffered, and he said that every now and again he felt a severe griping pain. On looking for a cause, I observed that about a foot of the lower end of the upper portion of the cut bowel passed into peristalsis each time the patient groaned. By pinching the bowel I could start the peristalsis, and the wide flaccid bowel would contract into a thick fleshy rod. Each time this happened the pain was felt, and on asking him where it was felt, he indicated the umbilical region. Here, before my eyes, was the undoubted cause of the pain, situated at some distance away from the place where the pain was felt.

After severe colic due to contraction of non-striped muscle (bowel, gall-duct, ureter, uterus) areas of hyperalgesia were frequently produced which often remained for a day or two after the exciting cause of the colic had been removed.

But I could not tell what other conditions were capable of producing these reflex phenomena. One observation which I think is true, in part at least, is that peritonitis, acute and chronic, can produce cutaneous hyperalgesia and muscle contraction—as shown by the "board-like" abdominal wall. In operating on ovarian cysts, for instance, if I found that the abdominal wall was not tender nor the muscle rigid, then I knew I would meet with no troublesome adhesions. If, however, the abdominal wall was hard and tender, I would certainly meet with adhesions. But these observations were too few to enable me to speak with assurance on this point. In gastric ulcer I lean to the view that the severe pain is really due to the contraction of limited portions of the muscle wall of the stomach, probably at the ulcer, partly because of its somewhat wave-like character, and partly because it is provoked usually at certain stages of the digestion, when the peristaltic waves are normally increased.

The difference between the production of pain by the heart and other hollow muscular organs.

The association of pain due to a stimulus provoked by the contraction of non-striped muscle, led to an inquiry into the production of pain in affections of the heart. It is well known that there are a great many theories which attempt to account for the pain in Angina Pectoris. It seemed that the only way to settle this question was to study carefully all the conditions present, and watch the individual for the remainder of his life, taking careful note of all associated phenomena. To begin with, it was soon apparent that though the fundamental cause of pain in a hollow muscular organ like the bowel was due to strong peristaltic contractions, this was not the cause in a hollow muscular organ like the heart, for the heart could be demonstrated not to pass into peristalsis when the pain was present. It was necessary to seek some other explanation, and this involved a long inquiry which properly falls under another section—that which deals with heart failure.

The summation of stimuli as a cause of pain arising from the heart.

Here, however, I may point out as a result of this part of the inquiry, I had concluded that the pain was demonstrably the outcome of exhaustion of the heart muscle in certain well defined cases. By its constitution the heart muscle is incapable of passing into a state corresponding to the strong peristaltic contraction of other hollow muscle organs, so that cause could be eliminated. The only other condition likely to produce it was a "summation of stimuli," i.e., the exhausted heart muscle produces by a series of beats the same stimulus that a long strong contraction produces in other hollow viscera. I put this view to Professor Sherrington. He

replied, "I have been thinking over your argument. I believe it is exactly right. Further points in its favour, in my view, are, that summation is a factor pre-eminently potent in production of most pains and, I expect, of all pains. For instance, a single induction shock to the skin that is not intense enough to cause any pain, becomes on repetition—as Ch. Richet pointed out long ago (and it is a good class experiment)—unbearable by summation. At a touch-spot induction shocks do not produce sensation at all easily; but at the so-called pain-spots sensations occur with peculiar ease and violence. Hence the tight hat or tight shoe is hardly noticeable as being 'tight at first,' but they soon become unbearable by summation."

While I do not consider that this view of pain from the heart is proved, the application of the view to treatment, and for the purpose of estimating the functional efficiency of the heart, has been of so much use that I am certain it will not be found far from the truth when the true cause of pain is revealed.

The stimulus adequate to produce pain.

The inquiry I pursued also included the testing of other hypotheses. The most common one is that pain, particularly visceral pain, is the result of "tension." But I could find no support for this. I could strain and press such an organ as the bowel and elicit no sensation, but as soon as a strong peristaltic wave was produced—as by an enema—pain occurred. It would seem as if there is required some vital process distinct from a passive one that can produce a stimulus adequate to excite the pain centres. So far as I know there is no explanation of the nature of the stimulus capable of producing pain by the viscera. Artificial

stimuli readily produce pain in the external body wall, but no known artificial stimulus can produce visceral pain without calling into play some vital process.

These illustrations and experiences which I have given in dealing with the reflexes—sensory and motor—caused by disease, show not only the need for more accurate information if we are ever to deal efficiently with the early stages of disease, but show also that there is a field of physiological inquiry necessary to this knowledge, which can never be worked out by physiologists if they continue to restrict themselves to laboratory observation. They suggest, indeed, that the surgeon must take a wider view of his profession and utilise his magnificent opportunities for advancing medical knowledge in this field.

CHAPTER III

THE INVESTIGATION OF IRREGULAR HEART ACTION

SHORTLY after entering general practice I had the misfortune to attend a pregnant woman who died undelivered on account of heart failure. After this melancholy experience I felt that this death might not have occurred if I had had a better knowledge of the condition. On studying the literature of the subject I found that sufficient knowledge did not exist. The best work on the subject was by Dr. Angus Macdonald, of Edinburgh, and the latest obstetric text-books show that no advance has been made since Macdonald published his book in 1878.

Finding the literature wholly unsatisfactory, I resolved to undertake an investigation. It struck me no one had a better opportunity than the general practitioner. So I studied the circulatory condition of women before pregnancy, watched them carefully during the time they were pregnant, observed them closely during labour and the puerperium, and for months and years after. I studied not only cases with damaged hearts, but also many healthy women. After several years I had collected a large mass of material. I found, among other things, changes in the size and position of the heart, murmurs of different kinds, variations in rate and rhythm, and other departures from what is usually considered the normal.

The nature of the problem in heart disease and pregnancy.

When I came to analyse my observations, I found a great variety of signs which needed interpretation

before I could find out what bearing they had on the matter. The essential problem had resolved itself into the question of heart failure. Pregnancy, in some, induced heart failure, so that I wanted to know, did any of these signs indicate heart failure, or did they foreshadow its occurrence? I could not answer these questions. I turned to my text-books for help, I ransacked libraries without avail, till at last it dawned upon me that the knowledge I wanted did not exist. Take, for instance, two groups of phenomena—murmurs and irregular action of the heart. Having detected a murmur or irregularity in a heart, I wanted to assess its value from the point of view of the patient's future, and its relation to heart failure. It was evident that no progress could be made to solve the problem till this was done.

Having had the lack of knowledge forced upon me in this way, I wondered if I could not do something to shed light upon this matter. When I started in a somewhat hesitating fashion on this quest I had no idea where it would lead me. Not only did it bring to light many new facts, but it brought to me a consciousness of the deficiency in our knowledge in fields essential to the progress of medicine, and a knowledge of methods by which some of these deficiencies could be made good.

When I found that I could not get any further in the inquiry as to the danger of pregnancy in heart disease until I was able to assess the value of the phenomena I detected, I took up each of them and subjected them to special study.

Method taken to appreciate the significance of irregularities

One of the most frequent abnormal signs in pregnant women was irregularity in the action of the heart, and the question of its significance naturally arrested my

attention, and I wanted to know its relation to heart failure. At that time, about 35 years ago (and even in much of the literature current to-day), irregular action of the heart was only referred to in the vaguest terms and always associated with some obscure suggestion of evil, as "the patient's heart became irregular and he sank and died." There was no differentiation of the different kinds on any sound basis, and terms such as intermittent, irregular, or irregularly intermittent were applied without discrimination to all kinds of abnormal heart action. To get a clear conception of the subject it was evidently necessary to see if the irregular action was always of the same kind or if there were different kinds. To enable me to do this I started collecting different irregularities by obtaining records with that simple but very efficient instrument, the Dudgeon sphygmograph. It was soon apparent from the radial tracings that there were undoubtedly several kinds of irregular heart action, but though I tried many ways to differentiate the one from the other, I could not get one that satisfied me till I sought for other evidences than the arterial pulse or heart beat.

The pulse in the jugular veins; how it was recorded and interpreted.

In following out one of the precepts that I had lain down for my guidance, namely, to seek out the significance of every sign that indicated a departure from the normal, my attention had been arrested by the pulsation in the jugular veins. As a matter of fact, while yet I was resident in the Edinburgh Infirmary, I had noted this pulsation and had called the attention of one of my teachers to a remarkable case where a large wave could be seen in the jugular vein, and asked him the meaning of it. He casually remarked that it was simply a "backwash from the heart." At the time

I took up this subject this was the attitude adopted in all the English text-books that made any reference to the subject. In my observations I was struck with the way the size of the pulsations and the distension of the veins varied during the various phases of pregnancy, labour, the puerperium, and afterwards. I made many attempts to obtain graphic records. My training had not given me any knowledge that could help, so I had to devise methods of my own. I tried to get the movements of the veins to record themselves by straws fixed upon the neck. Then I tried to conduct the movements from a funnel placed over the veins to a funnel covered by a membrane—at first a stiff membrane, then a yielding indiarubber membrane. I reduced the funnel to a small round box, and at last it dawned upon me I had re-invented a Marey's Tambour, which I had caught a glimpse of in my student days! I need not detail the steps I took after this. I contrived to employ the mechanism of the Dudgeon's sphygmograph to get a moving paper on which the lever could write, and by using a tambour which sat on the table, I was able to get a record of the pulsation in the jugular veins. The difficulty lay in interpreting the waves. I remember carrying about a tracing and asking physiologists to interpret it for me. Finding no one who understood it, I resolved to try myself. I saw it would be necessary to find what action of the heart caused the force which produced the waves. It was evidently necessary to obtain some standard movement taken at the same time so as to recognise the relation of the waves in the jugular vein to the movements of the heart. I obtained a physiological instrument with drum and tambour so that I was able to record at the same time the jugular pulse and apex beat. But this instrument was cumbersome and unwieldy when I had to take it about with me to the patients' houses, so I

was driven to devise a more useful instrument. It occurred to me that the radial pulse would form an excellent standard, so I made a tambour out of a pill box and fixed a handle to it of copper wire, and tied this to the upright stem of a Dudgeon sphygmograph. After a little manipulation I found it to work excellently, and from the crude model I got a very serviceable instrument made which I could carry about in my pocket. By substituting for the rigid band, which fixes the sphygmograph to the wrist, an elastic band which I tied in a knot, the instrument could be very

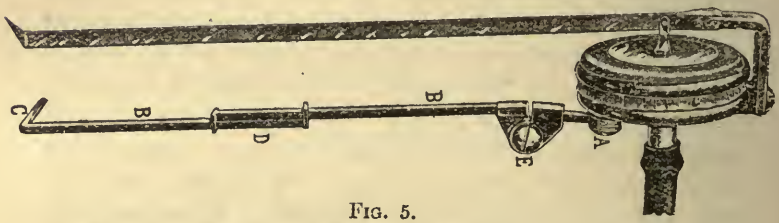


FIG. 5.

The tambour with sliding stem for attachment to the Dudgeon's sphygmograph.

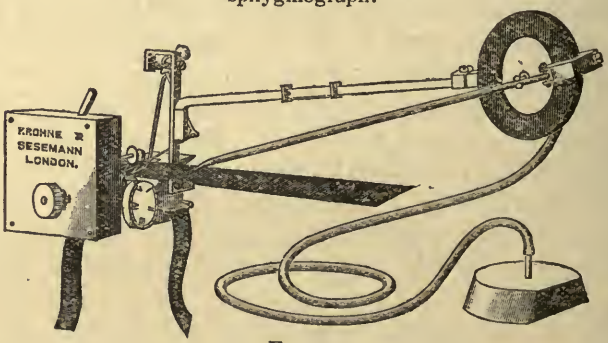


FIG. 6.

The clinical polygraph consisting of a tambour (Fig. 5) attached to a Dudgeon's sphygmograph.

rapidly fixed. So easy was this after a little practice, that I would take a tracing of the radial and jugular pulse or apex beat of a patient while the temperature

was being taken, so that it was not time-robbing. (See Figs. 5 and 6.)

The usefulness of the clinical polygraph.

This instrument I used for a great many years, and I look back upon it with a great deal of affection. It was both accurate and serviceable, and though it has been replaced by the ink polygraph and the electro-cardiograph, neither of these instruments are so adapted for general use. It was by means of this instrument that the foundations were laid for the accurate study of abnormal rhythms, a subject which had never been

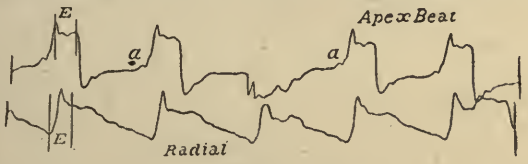


FIG. 7.

Simultaneous tracings of the apex beat and of the radial pulse showing the " systolic plateau " the space E, the period of the outflow from the left ventricle with the corresponding space E showing the effects of this outflow on the radial pulse. The wave *a* is due to the filling of the left ventricle by the systole of the left auricle.

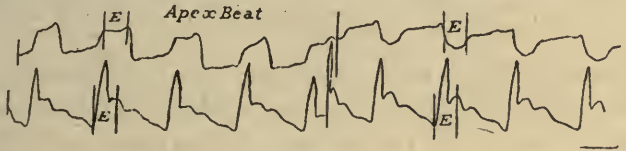


FIG. 8.

Simultaneous tracings of the heart movements (upper tracing) and of the radial pulse. The first part of the upper tracing was taken from the apex beat in the fourth interspace immediately outside the nipple, while the latter part was taken in the same interspace near the left border of the sternum. In the first part the cardiogram shows a " systolic plateau " during the ventricular outflow (E); in the other part the cardiogram is inverted, i.e., there is a depression during this period. (E.)

appreciated before, but which has opened up not only a new chapter in the diseases of the heart, but revolutionised the study and investigation of the physiology,

the pathology, and the therapy of the human heart. It was by this instrument that vague and indefinite notions concerning the significance of many obscure phenomena were rendered precise and intelligible, such as the nature of the movements of the different chambers of the heart, as shown in Figs. 7 and 8. Moreover, the character of the different forms of epigastric pulsation was clearly defined. For instance, the epigastric pulsation, due to dilatation of the right ventricle, was usually assumed to be of the same character as the apex beat—an outthrust during systole, while in reality it was exactly the reverse, an indrawing during systole

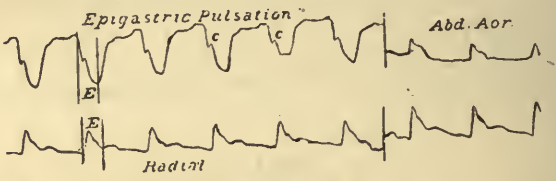


FIG. 9.

Simultaneous tracing in the first part of the epigastric pulsation due to a dilated right heart, and of the radial pulse showing a retraction of the epigastrium during ventricular systole (space E). In the latter part of the tracing are a few beats of the abdominal aorta, which are synchronous with the radial pulse, and have a different time from the epigastric pulse.

and an outthrust during deastole (Fig. 9). For many years it was taught by several authorities, that there was a delay in the transmission of the pulse wave from the heart to the peripheral vessels in aortic regurgitation. By this instrument it could readily be demonstrated that there was no such delay. Delay in the appearance of the left radial pulse was also supposed to take place in cases with an aneurism of the aorta situated beyond the origin of the innominate artery. This instrument showed there was no delay. By the use of this instrument the action of digitalis on the human heart was studied for the first time with

accuracy, and its methods of use intelligently described. The movements in the jugular vein could be registered at the same time as the radial artery, and the opportunity was thus afforded for studying the action of the right auricle and ventricle. While I shall deal a little more fully with this subject, the difference of the jugular pulse in Figs. 20 and 21 shows at once that there must be some remarkable change in the action of the heart to produce such different kinds of jugular pulse. The study of the liver pulse by means of this simple instrument also brought out the character of the different forms as seen in Figs. 22 and 23. The essential features of that condition, now called heart-block—which was so obscure until a few years ago—could be

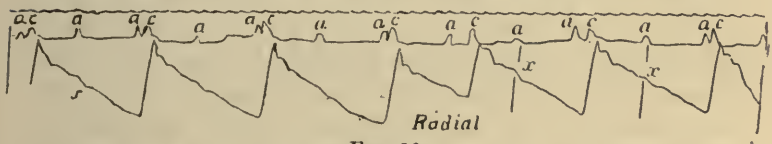


FIG. 10.

Partial heart block. This tracing was taken from a patient suffering from influenza. The upper tracing is from the jugular vein (wave *a*) and the carotid (wave *c*). The waves *a* are regular, and are not always followed by *c* waves, showing that the ventricles have missed a beat. In the radial tracing there is a depression (*x*) at the same time as the wave *a* in the jugular. This is due to the systole of the left auricle. In such a tracing we have evidence of the action of three chambers, the left auricle and ventricle and the right auricle.

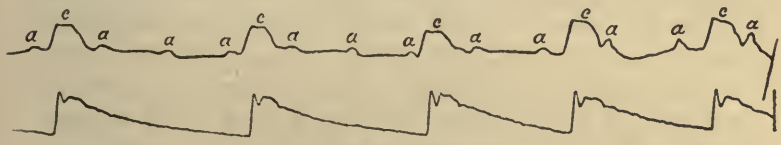


FIG. 11.

Heart block. The upper tracing is from the neck and shows the regular occurrence of auricular waves, while the carotid (wave *c*) and the radial (lower tracing) pulses are irregular and less frequent.

easily recognised in such tracings as Figs. 10 and 11.

I am aware that this description of the usefulness

of this invention will be considered as exaggerated, and I only indulge in it because I wish to emphasise the particular conception of clinical medicine which I have put in practice, and to show that very few physicians at this day realise what important observations can be made with a simple instrument which, because of its simplicity, has been derided as a toy, or ignored.

How the jugular pulse was used in practice and in research

By using the radial pulse as a standard, after a time I was able to locate each movement in the jugular vein and to recognise the forces which produced it. In the main this interpretation has been supported by experimental work. After I had worked out the interpretation, I searched systematically the literature of other countries, and found that already the subject had been imperfectly investigated, particularly by two observers, Potain in France, and Riegel in Germany. Their interpretation of the normal jugular pulse was in the main similar to mine, though there were a few points in which we differed. But where we differed most was in the application of these observations. These, and others, viewed the matter purely from an academic standpoint, and did not attempt to apply it in practice—or if they did no success attended their efforts. I attempted to apply this new knowledge in practice in various ways, at first seeking if there was any connection between the size of the waves with the state of the heart. It promised to be of use, for quite a number of my patients, who showed a large venous pulse, suffered from such exhaustion of the heart that they died. But continued observation showed that the size varied so much, even in healthy individuals, that no clear inference could be drawn, while in others about to die the jugular pulse disappeared altogether. It was usually faint or absent in elderly people. Finding

it of little importance as a prognostic sign, I tried to find out if its variations in size were in any way connected with changes in the heart. Here again only a limited success was attained. When a great increase in size occurred, if it was of a serious import, there were always other signs of cardiac inefficiency present, which gave a clearer indication. In the course of the inquiry it occurred to me to see what happened to the jugular pulse when the heart became irregular. When I look back upon the time when I conceived this idea, I realise that I had no notion of the great field that it opened up. From the outset of this new line of observation I was struck with the variations in the jugular pulse during irregular action of the heart. After a short experince I realised I had found a method by which I could attain the object which had baffled me for years—the differentiation of the heart irregularities upon a scientific basis—i.e., according to the mechanism of their production. It is unnecessary to enter into the prolonged inquiry which this subject required; suffice it to say that it is on the results of this investigation that the present classification of irregularities is founded. In after years other observers took up the subject, both by experimental and clinical methods, and supplemented the knowledge acquired by this simple method, though the results on the whole but confirmed those I had obtained.

Disadvantages of the clinical polygraph led to the invention of the ink polygraph.

The great drawback to the clinical polygraph was the fact that continuous tracings, extending over a long period, could not be taken. I had frequently great difficulty in obtaining a record of an irregularity that occurred at infrequent intervals, because the numerous short tracings would sometimes fail to catch

the occasional irregularity. In addition there were phases of abnormal action, such as paroxysmal tachycardia, whose onset and offset it was desirable to catch, which could only be done in a haphazard way by the clinical polygraph. In undertaking the investigation of the effect of drugs, which re-acted on the heart in the course of a few minutes or a few hours, it was necessary to obtain a continuous record of the heart's movements. For these and other reasons I felt that an instrument capable of taking continuous records, for hours if necessary, was desirable. It was evident that this could only be obtained by having a method that would write with ink, as it was impossible in clinical observations to use charcoal-blackened paper. I saw that a roll of paper on which writing could be made

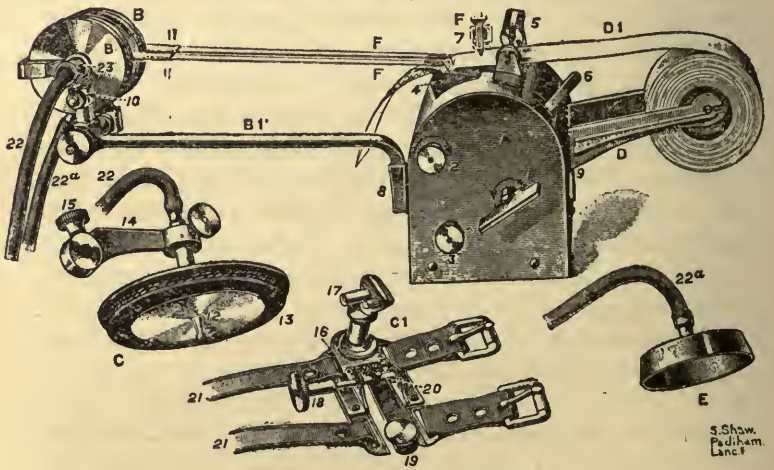


FIG. 12.
The Ink Polygraph.

with an ink pen would be necessary. I tried in a crude way to devise such a method, but being deficient in mechanical skill, I did not succeed. I was fortunate, however, in meeting a very intelligent and skilled

watchmaker, Mr. Shaw, of Padiham, Lancashire, to whom I communicated my views. He grasped my ideas at once, and became greatly interested in the undertaking, and between us, chiefly on account of his skill, we produced the ink polygraph—an instrument that fulfilled my ideal. (Fig. 12.) The usefulness of this instrument has been proved, and it has been found invaluable in many ways for research. When a drug is given, whose action is expected in a short time, as an atropine or strophanthin injection, the moment of response or the duration of the re-action can be studied with accuracy. The onset and offset of abnormal actions can be detected (see Figs. 13 and 14). On one

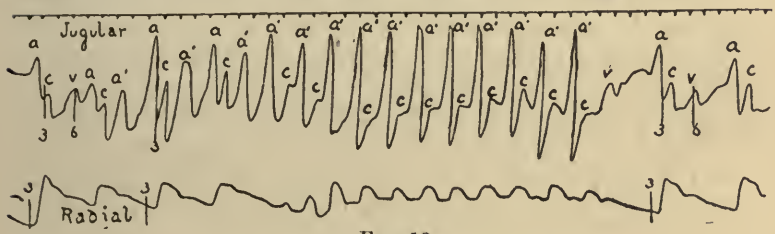


FIG. 13.
Showing a short attack of paroxysmal tachycardia.

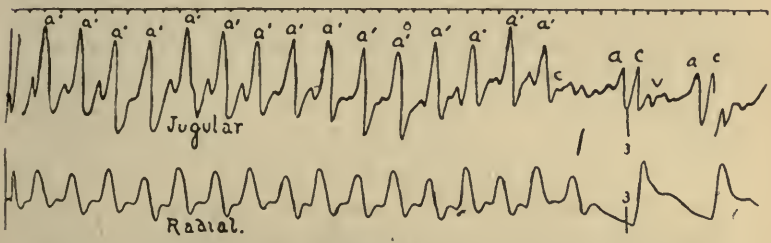


FIG. 14.
Showing the end of a long paroxysm of paroxysmal tachycardia.

occasion I took continuous tracings for 1½ hours, while the patient had 50 attacks of loss of consciousness of varying duration, due to temporary heart-block, and

I was thus able to recognise the effects of cerebral anemia in varying degrees of intensity. While I do not advocate the use of any mechanical device in routine practice, yet to a physician engaged in investigating diseases of the heart this instrument is so handy that it can be used with little trouble. In my consulting room I was accustomed to have it ready by the couch, and when the patient lay down, by simply applying the "receiver" or open cup over the jugular and carotid pulses, and moving the starting lever, a record could be got in a few seconds, and continued as long as one liked. This record gave the jugular and carotid pulses, and being versed with the meaning of the different waves, one had a record of three chambers of the heart—the right auricle (*a*), the left ventricle (the carotid pulse) (*c*), and the end of the systole of

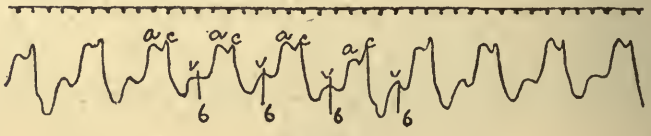


FIG. 15.

Tracing from the neck giving an accurate record of the movements of the jugular vein and carotid pulse, with the time registered in one-fifth of a second to show the rhythm and rate of the heart. From this tracing the action of three chambers of the heart can be perceived. The wave *a* is due to the systole of the right auricle, the wave *c* is due to the carotid impact (left ventricle). The wave *v* is due to stasis in the veins during the systole of the right ventricle. The down stroke 6, at the end of the wave, shows the time of opening of the tricuspid valves at the end of ventricular systole.

the right ventricle (end of wave v downstroke 6). (Fig. 15.) If the heart was irregular the character of the irregularity was shown, while the rate of the heart was accurately recorded.

This instrument is useful mainly in research work. For routine practice no instrument is necessary, and as soon as I had found out the knowledge it could

convey, I set about discovering means by which the information could be obtained by the unaided senses, to which I refer more fully later.

Assessing the value of symptoms.

Having a clear idea of the different forms of irregularity, I set about another piece of research, of whose significance I had little conception at first, as it was only after many years I realised its full meaning. This was to find out what happened to the patients who showed abnormal signs such as murmurs and irregularities—in other words, to assess their value so as to know what happened if the individual was untreated and allowed to lead his ordinary life, and thus find out if the cause of the irregularity needed treatment or if it were amenable to treatment.

It is one of the most remarkable things in clinical medicine, that the conception which is usually included in the term "prognosis" has never really been properly appreciated by the profession. We know that every writer on a disease has a chapter or a paragraph on prognosis; yet as I read it no writer has grasped either the full significance of the term or the manner in which the knowledge could be obtained. I deal fully with this in a later chapter, but here I may remark that I have frequently received unstinted praise and appreciation for the work I have done with those instruments, but I have never yet come across any reference or appreciation of the most important piece of investigation I have undertaken, a piece of research that has caused me far more trouble and labour, and taken up far more time than any other piece of work I have ever done. None of those who have, independently of me, pursued the same line of investigation, as to the mechanism by which irregularities are produced, have seriously attempted to assess their value. Most writers

not only neglect to take this step, but actually assume it to be so easy that the knowledge can be obtained with little trouble, so that I find this curious condition, that men who scientifically investigate the mechanism of the phenomena, are quite content with a species of guesswork as to the value of the phenomena.

I refer to this matter repeatedly and with insistence, because it is of vital importance to clinical medicine, and the profession universally have totally failed to grasp its significance; and until this field has been properly investigated, medicine will be imperfect as a science, and ineffective in its practice.

The real problem in assessing the value of a cardiac sign is its relation to heart failure.

When I determined to find out what bearing the cause of the irregularities had on the patient's future health, I did not know how to set about the work, but gradually I obtained an insight into the meaning of the problem and the method by which it could be pursued. The real problem came gradually to light, and it was the relation of the abnormal sign to heart failure. When I put to myself the question, "what is heart failure, and what are the signs by which it can be recognised," I found my notions very hazy. I had imbibed the notion that the heart failed by what is called back pressure; thus, the mitral valve leaked and so caused embarrassment of the left auricle, which became embarrassed in its turn, and so engorged the lungs, which again impeded the work of the right ventricle, which dilated and so caused tricuspid regurgitation and the end of things. This crude conception is held, more or less completely, all the world over, and if one cares to see the attitude of authorities, one can turn to text-books and try to find out what the author's conceptions are as to heart failure. Beyond a refer-

ence to back pressure signs he will find, that, though the book is intended to deal with the subject, little or no attempt is ever made clearly to define what is meant by the term "heart failure," or to give the symptoms by which it is recognised, except when it occurs in its grossest form, as with dropsy.

Method of research to find out the value of irregular action of the heart.

One can easily understand with the vague conception of " back pressure," assumed as the cause of heart failure, how a murmur has gained that grim significance which oppresses so many doctors at the present day. The conception was held that a heart, to be normal, must be regular, and that an irregular heart was an impaired heart, with which something tragic was associated. Having obtained the means of differentiating the different irregularities, I collected a large number of cases showing different forms of irregularity.

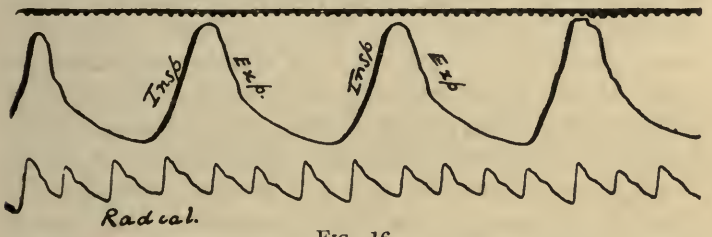


FIG. 16.

Youthful type of irregularity showing respiratory variations—diminished rate during expiration and an increased rate during inspiration —more marked with deep breathing (see Fig. 16A).

These I analysed in different ways, such as the mechanism of their production, the age at which they occurred, the state of health of the individual at the time. I found that the vast majority fell into three very distinct groups. There was one form which I found mainly in the young, in which all the chambers of the heart participated, to which I gave the clinical

title of the " youthful irregularity " (Figs. 16 and 16A). A second form occurred in which the ventricle contracted prematurely, at the same time as, or a little before, the auricle—which is now called the ventricular extra-

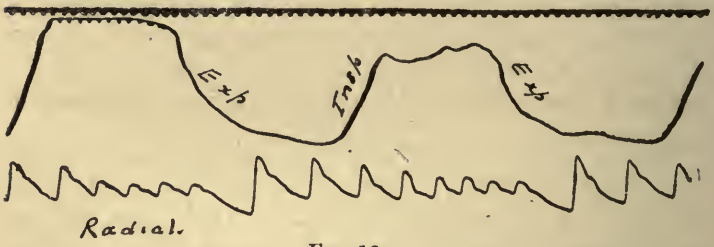


FIG. 16A.

Continuation of Fig. 16 when the patient breathed deeply and slowly. The increase in rate during inspiration, and decrease during expiration, is well marked.

systole. (Figs. 18 and 19.) I found that sometimes the auricle contracted prematurely—now called the auricular extra-systole. These premature beats or extra-systoles usually occurred at rare intervals, but sometimes they occurred with great frequency, and after every 4th, 3rd, or 2nd beat, or even an extra-systole would alternate with a normal beat—(pulsus bigeminus). I found this irregularity seldom in the young under 20, frequently after 40, and nearly everyone had it rarely or frequently after 60. For this reason, to separate it clinically from the first type, I called it the " adult type " of irregularity. The third form of irregularity was not nearly so frequent in occurrence as the other two. There the rhythm was very disorderly, and it was that form which we now recognise as due to auricular fibrillation, about which I shall say more presently.

The condition of the heart in these people formed a special object of study. I found the youthful type most distinct in perfectly healthy children and youths. When the heart slowed down after a febrile attack, as measles, it was very often distinct, but even in

healthy youths, accustomed to play strenuous games, it was often very marked. I watched those who showed this irregularity grow into manhood and womanhood, and observed many during temporary illness and during periods of severe bodily effort. They never

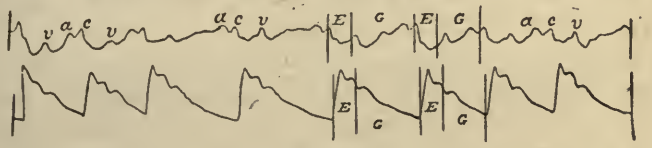


FIG. 17.

Simultaneous tracings of the jugular and radial pulses, showing the agreement in rhythm of the right auricle and ventricle (waves *a* and *v*), with the radial pulse, in the sinus or youthful form of irregularity. The irregularity is seen to be due to variations in the length of the diastolic period (Spaces G).

showed any signs of cardiac weakness even when they were engaged in hard manual labour. I therefore concluded that this condition was a physiological one.

The extra systole gave me a great amount of labour to find out whether it was a sign of any serious moment.

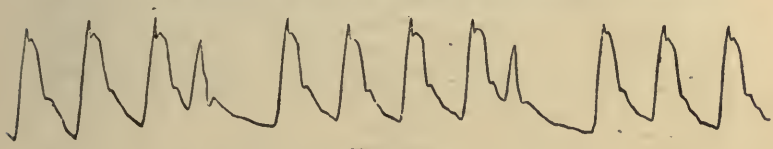


FIG. 18.

The small beats are due to extra-systoles.

As I found it in apparently healthy people engaged in work that entailed severe bodily labour, its cause did not seem to impair the heart's strength. I found it disappeared when the heart's rate increased during a febrile illness, and that it sometimes appeared during a febrile attack. While thus engaged I met with several patients, mostly elderly, who developed this form of irregularity during acute febrile illnesses, such as bronchitis and pneumonia, and who died. I then considered that its occurrences with febrile conditions was

of serious significance, and this view held good for about three years after which I began to meet with cases that recovered and so found the sign of little help. After watching large numbers of people for many years, I came to the conclusion that this irregularity was, as a rule, but one of the changes that accompany advancing years, of little importance, in otherwise healthy people, and when associated with signs of heart failure, the conclusion as to the heart's condition should be independent of this irregularity. There were a great many other features which cropped up in the course of this quest which I have incorporated in my writings elsewhere.

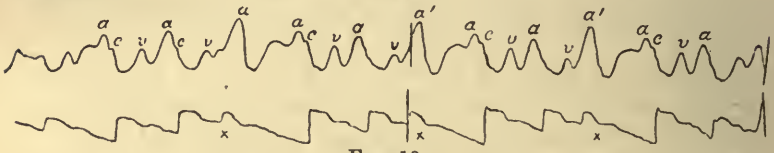


FIG. 19.

Simultaneous tracings of the jugular and radial pulses. The small beats x x x are extra-systoles. The auricle preserves its rhythm during the irregular periods in the radial pulse. The wave *a'* is the auricular wave during the premature contraction of the left ventricle. The absence of the ventricular wave *v*, after the wave *a'*, indicates that the right ventricle had contracted early, evidently synchronous with the premature contraction of the left ventricle, the large wave following *a'* being due to stasis.

During this time I was also inquiring into the significance of other signs, especially murmurs, and gradually I began to obtain a clearer view of how heart failure developed and of the signs by which it could be recognised. These results I have dealt with elsewhere, and out of them I developed a scheme for estimating the functional efficiency of the heart to which I will refer later. This study also enabled me to see *symptoms* from a standpoint which led to a recognition of their meaning and significance, on which could be based a sound and useful classification, a subject with which I deal in a subsequent chapter.

CHAPTER IV

THE RECOGNITION OF AURICULAR FIBRILLATION

In the last chapter I referred to the three kinds of irregular action of the heart that were most common. One was predominantly in the young, the other predominantly in the elderly, and both were perfectly consistent with robust health. The third common kind I

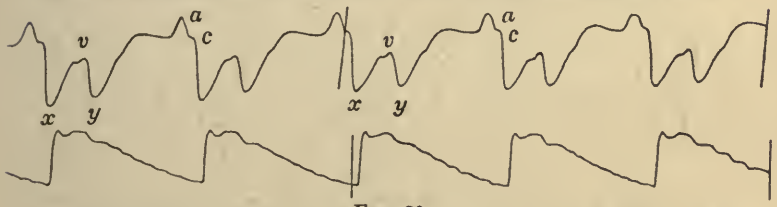


FIG. 20.

Simultaneous tracings of the jugular and radial pulse. The jugular pulse is of the auricular type, showing the wave *a* due to the systole of the auricle. The ventricular wave *v* is small and appears only at the end of ventricular systole.

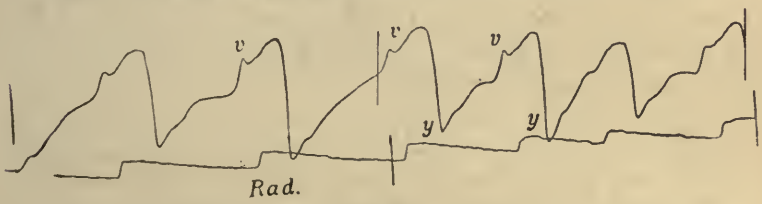


FIG. 21.

Simultaneous tracings of the jugular and radial pulse. The jugular pulse is of the ventricular type. The wave *a* is absent, and the wave *v* is so large that it fills up the whole period of ventricular systole, being in fact, due to the systole of the right ventricle.

found, mostly, in adults, who suffered from distinct evidences of heart failure. In the early days of my research into the venous pulse I had found that in most

tracings there was a wave due to the auricular systole (wave *a*, Fig. 20), but there were certain people whose jugular pulses showed an absence of this wave (Fig. 21). This difference led me to describe the jugular pulse as occurring in two forms, (1) The "auricular form," where there was a wave due to the auricle, and (2) the "ventricular form" of venous pulse, where there was no wave due to the auricle. I found that pulsations of a similar kind could also be detected in the liver where there was enlargement of that organ from heart failure. (Figs. 22 and 23.) These pulsations in the jugular had

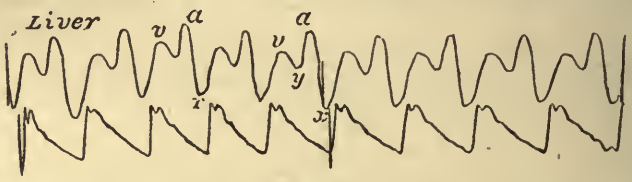


FIG. 22.

Simultaneous tracings of the liver and radial pulses. The liver pulse is like the auricular form of jugular pulse and shows the two waves *a* and *v*.

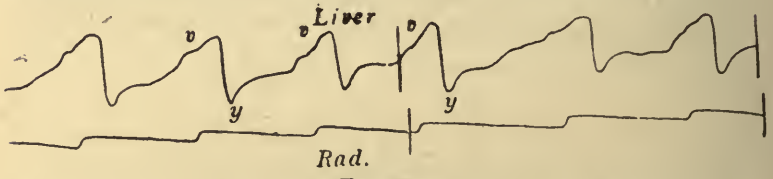


FIG. 23.

Simultaneous tracings of the liver and radial pulses. The liver pulse is like the jugular pulse in Fig. 21, being of the ventricular form, and shows no auricular wave.

been described before by others as the negative venous pulse, and the positive—the former being the auricular form, because there was a collapse of the vein during the ventricular systole, while the positive was the ventricular form, as there was a wave in the vein during ventricular systole. Their significance as well as the nature of the difference, however, had not been under-

stood. I preferred my classification which has been adopted in medical literature as it brought out the very important fact, that in the one case the auricle was active, while in the other there was no evidence of auricular activity, an observation that ultimately led to a very valuable discovery. I had also noted that the heart was, in the great majority of cases, irregular in its action, quite distinct from any other form of irregularity, being so disorderly in its rhythm, that often no two beats following one another were of equal size, nor the intervals between the beats of equal duration. (Fig. 24.)

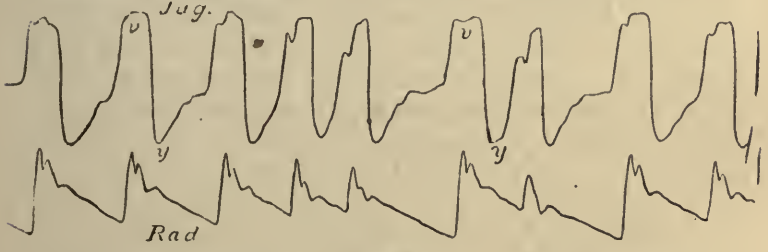


FIG. 24.

Characteristic irregularity and jugular pulse of auricular fibrillation.

From amongst my patients, I found that there were two distinct factors associated with this irregularity, it occurred most frequently in elderly people, and in others who gave a history of rheumatic fever. Moreover, I found it present in at least 80 per cent. of my patients who suffered from heart failure, with the symptoms of dropsy and enlarged liver.

The first case in which the onset of Auricular Fibrillation was recognised.

To get an idea of the meaning of these changes it was necessary to see the condition at the onset. I therefore set myself the task of watching people whom I had attended during attacks of rheumatic fever, and

people getting on in years. I had to wait and watch several years before I obtained a satisfactory result, but when it came there was revealed far more than I expected. The first patient in whom I detected the onset of auricular fibrillation was a woman, whom I attended in an attack of rheumatic fever in 1880, she then being 31 years of age. She had had a previous attack of rheumatic fever at the age of 22, and had a presystolic murmur in 1880. I attended her for attacks of rheumatic fever also in 1883 and 1884. I saw her at frequent intervals for various complaints as she showed symptoms of angina pectoris and gastric ulcer. In 1892 I detected an enlarged and pulsating liver, tracings from which were of the auricular form of liver pulse, and similar to that of the jugular pulse. This type of pulse, as shown in Figure 25, persisted in both

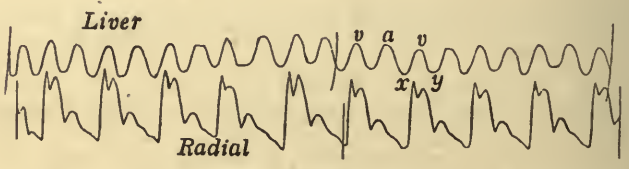


FIG. 25.

Showing the auricular form of the liver pulse in a patient with a mitral and triscuspid stenosis, before the onset of auricular fibrillation. There are two waves *a* and *v* during the cardiac cycle.

jugular and liver, with a presystolic murmur until 1898, when she was suddenly seized with great breathlessness, and the heart became rapid and irregular. Under digitalis a slowing of the heart took place, accompanied by a considerable improvement. When the heart slowed down it was persistently irregular, and the jugular and liver pulses, instead of being of the auricular form, were now ventricular, i.e., there was now no wave due to auricular systole, as can be seen by

comparing Figure 26 with Figure 25. But still more striking to me at the time was the total disappearance of the presystolic murmur, which I had heard at every examination for eighteen years. She lived for a year

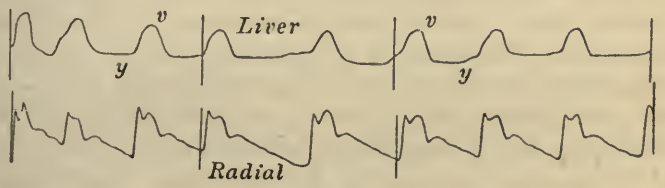


FIG. 26.

Shows the liver pulse—ventricular form—after the onset of auricular fibrillation. There is but one wave (*v*) during the cardiac cycle, the wave *a* having disappeared, and the presystolic murmur had also disappeared.

after this and I made numerous observations, but there was never a return of the presystolic murmur, nor of the auricular wave in the jugular and liver pulses. Here was undoubted evidence of a disappearance of all signs of activity of both auricles, and as at the post mortem examination the auricles were enormously dilated and the wall atrophied, I provisionally adopted the view that the auricles were paralysed, and under the name of Paralysis of the Auricle I described the condition in my book on the pulse, published in 1902.

The effect of Auricular Fibrillation on the heart's activity.

Having thus obtained data sufficient to recognise the condition as a distinct clinical entity, I devoted much attention to studying the heart's efficiency as affected by this change. Gradually I detected the condition in a considerable number of my patients. Many were in fair health till its onset, when they became breathless, and showed a variety of signs of heart failure. In a few death speedily followed, others remained for years in a crippled condition, so that I obtained a fair

conception of the signs of oncoming heart failure. Thus in one of my patients the "auricular paralysis" occurred in periodic attacks, lasting from a few hours to a few weeks. I closely observed her during the attacks and saw the signs of heart failure gradually develop, with breathlessness, cyanosis of the face, dropsy, and dilatation of the heart. With the sudden reversion of the heart to the normal rhythm, all these signs disappeared in a few hours, and the patient would be able to be up and about her household duties. I noted that the rate of the heart, during the attack, was always rapid as well as irregular. I watched her for years during numerous attacks until the condition became persistent, with gradual drifting to death. I studied the effects of many remedies, and finally had the heart carefully examined post mortem.

The significance of a slow action in Auricular Fibrillation.

In another patient, whom I attended for rheumatic fever in 1883, then aged 32, there was left a systolic murmur. In 1892 I discovered he had a presystolic murmur, separated from the first sound by a slight interval, and I found in his jugular tracing that the time between the auricular wave in the jugular pulse and the carotid pulse was also slightly increased (a prolonged a — c interval indicative of a slight damage to the auriculo-ventricular bundle). This patient had a bad breakdown from heart failure in 1897. From this he recovered, and in 1904 he was following his trade as a mechanic. I had kept in touch with him, seeing him at frequent intervals, and one day I found his pulse irregular and slow, and the auricular wave gone from the jugular pulse, and a total disappearance of the presystolic murmur. This condition persisted for a week, then the heart became regular and the presystolic murmur and the auricular wave in the jugular

pulse reappeared, indicating that the normal rhythm of the heart had returned. The normal rhythm continued till November of that year, when they again disappeared, the abnormal rhythm continued till he died, in October, 1914. An electro-cardiogram taken in 1910 confirmed the absence of auricular contractions. The point that arrested my attention in this case was that he was not conscious of the change in his heart's condition and there was no sign of heart failure, and he could follow his work as a mechanic all day, and even work overtime with no undue fatigue. The rate of the heart with the normal rhythm was about 60, and there was no increase in the rate with the onset of the abnormal irregular rhythm.

The significance of the action of digitalis in Auricular Fibrillation.

It occurred to me that probably the heart failure in the other cases was due to the excessive rate of the ventricle, the ventricle being exhausted by a want of rest. This seemed also borne out, for when the heart in these cases slowed down by digitalis, improvement occurred, while others in whom digitalis did not cause this slowing, drifted to death. I then began the study of digitalis in these hearts, and found that the majority were very susceptible to the drug. So long as the heart beat at a rate under 80, they were fairly well, whereas when the rate exceeded 110, they gradually showed increasing signs of heart failure. I therefore tried in each case how much digitalis was required to keep the heart in check, and was frequently able to regulate the dose to that amount, which kept the rate under 80, and thus enabled the patients to pursue their occupations for years, though at a lower level than in health.

Some results of the study of cases of Auricular Fibrillation.

The study of these cases gave me quite a new conception of heart failure and how it was brought about, and demonstrated that in the cases supposed most typical of "back pressure" (where there was dropsy and enlarged liver from heart failure) it was not brought about by the giving way of one valve and chamber after another, but was induced through the inception of this new rhythm. It also gave the data for a sound prognosis, for it was found that in those cases where the rate did not increase, the heart's efficiency was little, or not at all, impaired. When the rate was persistently rapid, heart failure of a severe type set in sooner or later. If the rate could be slowed and kept down under digitalis, the outlook became hopeful; if this could not be done, then the outlook was grave. The post mortem examination of these grave cases always revealed that in addition to the abnormal rhythm there were other complications as myocardial degeneration or aortic regurgitation. Other curious results come out, as for instance that angina pectoris rarely occurs in these cases, and if a patient with angina pectoris develops this abnormal rhythm the attacks of angina pectoris cease.

The discovery of the nature of the abnormal rhythm.

On looking at the heart, post mortem, in one of these cases, after I had watched the onset and continuance of this condition for seven years, I found that the muscle wall of the auricles was hypertrophied. Manifestly, an organ that was paralysed for seven years would not also be hyperthrophied, so I reasoned that paralysis of the auricle would not account for the condition. It seemed to me, that if the auricle contracted, it must have done so during the ventricular systole, for neither the polygraph nor the electro-car-

diograph revealed any evidence of its contraction during the pause between the ventricular contractions, even when the rate was 50 per minute. I therefore assumed that the auricles and ventricles contracted together, and that the stimulus for contraction, in place of arising normally at the mouth of the superior vena cava, arose at the node of tissue situated at the beginning of the bundle which conveys the stimulus for contraction from auricle to ventricle, and hence I provisionally called the condition "Nodal-Rhythm." I was never fully satisfied and sought to get a clearer conception of what was really happening. In 1906 Cushny and Edmunds drew attention to the resemblance of the radial tracings, in a case of paroxysmal irregularity, with the tracings from a dog, in whom they had produced fibrillation of the auricles. I considered the idea for a time, and thought that probably this was the condition in these cases of auricular paralysis or nodal-rhythm, and even went so far as to publish tracings in 1907, showing small waves in the jugular tracing which I attributed to a fibrillating auricle. But I must confess I never really grasped the full meaning of Cushny's and Edmunds' discovery, and harped back to the old idea of a nodal-rhythm. It is sometimes surprising how one will be in touch with one aspect of a subject and fail to appreciate its other qualities. My ignorance of physiological experiment—I never having witnessed the heart in this condition—doubtless accounted for my blunder. Still I kept on trying to get a better understanding of the subject, and later when I went to London, Dr. Lewis became interested in the subject, and he produced auricular fibrillation in a dog, and took records of the arterial and venous pulse and demonstrated their identity with similar tracings in man, and showed that the condition in man was undoubtedly due to auricular fibrillation.

The bearing of the recognition of Auricular Fibrillation on Medical Research.

As years go by, and as physicians become more expert in detecting this condition of the heart and all its symptoms, it will be more and more recognised that its discovery forms a landmark in cardiac clinical pathology. I may be excused if I insist upon the fact that all the essential details associated with this condition, its symptomatology, its relation to heart failure, its response to digitalis, were all discovered by the simple means available to a doctor in general practice. Moreover, and this is the point which I wish to make, the recognition of this condition as a clinical entity and all it implies could not have been made out by investigators however capable, if they had restricted themselves to laboratory or hospital ward.

CHAPTER V

MITRAL STENOSIS

Among other issues that required investigation was a knowledge of the prognostic significance of murmurs. How far we still are from realising their significance is manifest from the universal confusion which is exhibited in everyday practice and from the fact that the method by which knowledge might be obtained is not understood. I deal with this later. Here I wish to give a specific instance of the methods employed. Mitral stenosis for 50 years has been recognised by the murmurs it produces, and it is tacitly assumed that this subject is already sufficiently understood. Yet this is far from being true. We know that the mitral orifice in this disease frequently undergoes progressive narrowing, till the opening becomes so small that the supply of blood to the body is reduced below that which is necessary to carry on life. The rate at which this cicatricial process goes on varies, as also the extent which it may reach. In order to determine the prognostic significance of mitral stenosis, it is necessary to find out the extent of the narrowing, and the rate at which it is progressing. For instance, in a case of pregnancy, the question whether the heart may be fit to bear the strain may depend upon the extent of the embarrassment offered to the blood flow by the narrowed orifice, so that the degree of narrowing, and the rate at which it is taking place, are questions of prime im-

portance. Nevertheless, even in the most recent literature there is shown no realisation of the importance of this matter nor any hint that there are signs which afford this necessary information. In the observation of cases of rheumatic fever, I found that none ever showed any trace of the presystolic murmur, which we recognise as a sign produced by stenosis of the mitral valve during the attack of fever. I was occasionally surprised to detect a presystolic murmur some years after an attack of acute rheumatism in cases that I knew had not shown it in previous examinations after the attack. When one reflects upon the matter, the reason is obvious. An inflammatory affection of the mitral valve in the acute stage can cause no narrowing, rather it may destroy a portion of the valve and render it incompetent. The narrowing can only arise when the inflammation subsides, and the cicatrising process sets in. This accounts for the fact that we may detect a systolic murmur during an acute attack of rheumatic fever in which the heart is affected, but never a presystolic murmur, due to stenosis of the valve, unless the heart had been previously damaged. A systematic study of the individual cases of rheumatic fever that came under my care showed that the evidence of stenosis did not appear for years after the causative inflammation. The first sign of the stenosis was a thrill at the apex with the auricular systole, later there appeared a short presystolic murmur. This murmur at first was not always present, but later it became constant. As time went on, this murmur increased in length. There next appeared a reduplicated second sound, and later a short murmur immediately after the second sound. This murmur gradually increased in length, till it merged in the presystolic murmur, so that the long interval between the second and first sounds was sometimes filled with murmurs. Now, all

these variations indicate a progressive stenosis. If the date of the causative attack of rheumatic fever can be ascertained, an idea, rough indeed, but valuable, of the rate of progress of the stenosis, can be obtained. Thus, all these murmurs may be present in four or five years after the causative attack, indicating that the stenosis is proceeding at a rapid rate, while in others there may be but a short presystolic murmur, ten or twenty years after. Needless to say, accompanying these signs there are always other signs, indicating degrees of heart inefficiency.

In any given case of mitral stenosis, where the question of pregnancy—or indeed, where prognosis of any kind is required, the search for these signs will lead to a recognition of the stage of stenosis which has been reached, and a rough knowledge of the rate of cicatrization can be obtained of real practical value.

If the observation on these patients be pursued long enough, a great variety of symptoms may be perceived. In the majority of cases the pre-systolic murmur will suddenly disappear, and there will only be left the diastolic murmur, indicating the onset of auricular fibrillation.

In other cases curious changes take place in the rhythm of the heart, showing that the damaged valves are but a portion of the mischief done by the rheumatic fever, and digitalis may produce a variety of different irregularities. One of these peculiar changes is sometimes the appearance of a mid-diastolic murmur. Its place in the cardiac cycle is between the second and first sounds of the heart, during the diastole of the ventricle, and distinct in time from the pre-systolic and diastolic murmurs just described. On taking tracings of the jugular pulse it is found to occur at the same time as the auricular wave in the jugular pulse, and in fact it is due to the auricular systole. The reason

for its separation from the first sound is because there is a delay in the stimulus for contraction passing from auricle to ventricle (Fig. 27). Further remarks on the subject are made on page 130.

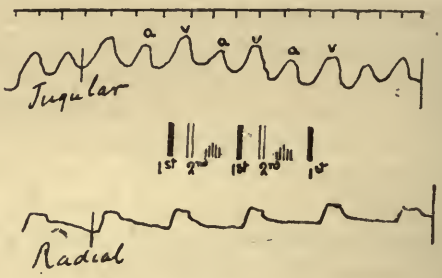


FIG. 27.

Tracings of the jugular and radial pulses in a case of mitral stenosis. The jugular shows the two waves due to the auricle (*a*) and ventricle (*v*). Between the tracings is represented diagrammatically the time of the first and second sounds and the mid-diastolic murmur. The latter is seen to occur at the time as the auricular wave in the jugular tracing.

CHAPTER VI

OBSERVATIONS ON THE EFFECTS OF DRUGS

In no branch of medical research is there such a necessity for the understanding of the symptoms of disease as in that concerned with the effect of remedies. Everyone can see that before you can rationally treat a patient, you must recognise the nature of his illness. The most prominent sign is often taken as an indication for treatment, and it is rare that an attempt is made to understand the real significance of the sign. Here and there no doubt an attempt, sometimes successful, is made to find the cause and remove it, but that rarely means the giving of a drug. I do not exaggerate when I state that not one of the numerous drugs that receive official sanction, and are incorporated in the pharmacopœia, have been studied with that care and intelligence which are necessary to the understanding of their effect. Even those drugs whose action is of undoubted benefit have not been studied with that care and accuracy which should bring to light the properties that render them beneficial.

The effects of remedies should be studied in the diseased individual. His symptoms should be carefully noted, and their origin recognised. The effect of the remedy should be watched by observing how the symptoms are modified, and the new signs that the remedy provokes should be detected. This procedure necessitates in the observer a preliminary knowledge of the symptoms of disease.

The neglect of this method of studying the effect of

remedies accounts for the disappointment caused by many remedies whose discoveries were heralded as at last bringing relief to the suffering and foreshadowing the cure of disease. One has but to reflect on the enthusiasm with which electricity, the X-rays and radium were first introduced, or the extravagant hopes that were raised on the introduction of tuberculin. To-day the same premature enthusiasm is aroused over the introduction of Salvarsan. While one cannot but admire the patient research which resulted in its discovery, its success will be marred for the want of an intelligent interpretation of the symptoms produced by syphilis. Already its limitations are becoming evident, partly on account of a lack of discrimination of cases that are suitable, and not suitable, for this treatment.

Before we can say that a drug is effective as a remedy for such a disease as syphilis, it is necessary that the individuals who have been treated should be watched for the remainder of their lives. This may be looked upon as impossible, and put out of consideration. The inability to see the need for it is an illustration that the principles of clinical medicine have never been grasped. Clinical medicine demands the observation of cases extending during the whole life of an individual, however long he lives. It is well known that the symptoms of syphilis may not appear till a great many years after the primary infection. How, then, can anyone be sure of the effect of the drug, until a number of individuals have been observed for a sufficient length of time? It is true that the observer's life is too short. The man who plants an acorn does not expect to see the full grown oak. One may plant and another may water, but neither may reap the increase. The true observer is he who is content to do the spade work, indifferent as to who shall realise the result, so long as the aim of medicine is achieved.

I resolved to try to see even with my limited resources if the action of remedies could not be more accurately investigated, and those of distinct value recognised, and their method of administration adapted to individual requirements. I had, however, a great difficulty in knowing how to begin, and spent much time in futile endeavours. Thus, in seeking for evidence as to the effects of tonics, I could get no demonstrable signs, and the patients' impressions were so liable to suggestion, that they could not always be relied upon. Having a large number of people with digestive troubles under observation, as can be inferred from Fig. 1, page 4, I resolved to see if I could get a clearer idea of the action of remedies on the digestive tract. Most of the patients presented few or no physical signs, so one had to depend upon the description of their sensations, and that required a good deal of discrimination. For instance, flatulence was a common complaint, and one had to get a clear idea of what each patient meant by such an expression as "wind on the stomach." A careful inquiry elicited the fact that a considerable proportion of the patients who complained thus were really air swallowers—a habit of which many were unconscious. Eructation and heart-burn were common complaints, and benefit usually followed by taking soda and bismuth. A study of the circumstances under which heart-burn appeared, its relation to the time of taking food, and the kind of food, showed that there were different varieties. As many were food-bolters, or ate and drank at the same time, their symptoms could be relieved and permanently cured by getting them to take dry food in small mouthfuls, and performing thorough mastication. I tried to understand the real cause of the complaint, but my lack of knowledge of diseases of the digestive tract was too great to enable me to get far. The action

of aperients is a subject which forces itself upon every practitioner, and I tried to understand the effects of the different kinds.

It soon became apparent that constipation was a far more complicated matter than I had suspected. The fact that people react differently to the various aperients shows that there must be factors concerned which we do not yet recognise. This is manifestly the case, for constipation occurs associated with diseases at different levels of the intestinal tract. A very instructive line of observation was found, in noting the action of aperients, and I think it would be a useful method to adopt in teaching, for students to note the effects of simple aperients on the patients who have to take them. This has never been done even by experienced physicians, and it is wonderful what light can be thrown upon the symptomatology of the intestinal tract in this way. For instance, the time that an aperient takes to act is of importance in recognising the rate of intestinal movements in the different diseases. The number of evacuations, and their character indicates the height at which the scybala may have lodged. The sensations evoked in the patient—the feeling of collapse, at times allied to shock, and the phenomena produced on other organs as the heart and blood vessels should be observed. The appearance of pain indicates that peristalsis has occurred, and the situation of the pain, and cutaneous hyperalgesia and hardening of the abdominal muscles reveals the portion of bowel which has passed into peristalsis.

The need for such methods of observation may be illustrated by the following experience. A young lady patient of mine suffered from constipation, and suffered severely when the scybala were expelled by an aperient. Being in London on a visit she was seized with an alarming illness, and a distinguished physician was

called in, who was also a pharmacologist. He wrote me a long account of the girl's grave condition. He had found great tenderness of the abdomen and a board-like contraction of the abdominal wall indicating clearly an attack of peritonitis. He recommended that the girl should lie in bed for six weeks and have suitable treatment, and that then if no better she should have an abdominal operation in order that the adhesions should be broken down. When I saw the girl I found she had one of her usual attacks of constipation, and the aperient she had taken had caused the shifting of the scybala with the usual pain. An instructive commentary could be made on the fact that physicians learned in the action of remedies have never studied the effects of a dose of castor oil on a constipated girl.

I next resolved to test the value of heart drugs, as the means I had of recording the action of the heart permitted the recognition of any modification in the heart's activity. I began with the drugs that were reputed to have a speedy effect. Strychnine at that time had a great vogue, its effects being insisted upon by physicians and surgeons—the latter, indeed, often refusing to operate on a patient under an anæsthetic unless the patient had a preliminary dose of strychnine, while anæsthetists had often beside them a hypodermic syringe ready charged lest the heart should fail. To my surprise I could get no result whatever. In medicinal doses, by the mouth or by hypodermic injection, no effect could be traced in the healthy heart, nor in people in a state of collapse. I read up the literature, and beyond assertions as to its value, there was not on record a single instance where there was given any evidence to justify the belief in its properties. Later, in the London Hospital, a series of careful observations by Dr. Parkinson confirmed the fact that

in medicinal doses strychnine has no effect upon the organs of the circulation. The same results followed an investigation into the action of caffeine and oil of camphor. Aconite was another drug that had a great reputation as a "depressant of the heart," but it was also found to be without any effect in medicinal doses.

Time and again I had witnessed undoubted effects produced by digitalis, and I carefully noted the cases to whom I gave the drug. Following the usual practice, I gave it to all and sundry who had or were supposed to have an affection of the heart, and kept a note of their reaction or want of reaction. After ten years' observation, I collected all the cases to whom I had given the drug, and analysed the result. In the vast majority of cases I could get no evidence of its effect upon the heart, and the question arose whether the quality of the drug, the insufficiency of the dose, or the patient's insusceptibility was the cause of want of action. It was my custom to push the drug until I had got sufficient evidence that the drug was active, the evidences of activity being shown by some action on the heart, or by nausea—sometimes with vomiting—or diarrhœa. The heart, in those in whom it had an effect, presented a curious variety of phenomena. In some, with a normal rhythm, the heart's rate slightly decreased, the slowing sometimes appearing at intervals, for a few beats; in others, extra systoles occurred. In quite a number the pulse became irregular, due to the dropping out of ventricular beats (partial heart block), while the auricles were unaffected. In rare cases, with diseased hearts, peculiar reactions developed, as a slowing down of the whole heart, with the ventricle starting off independent of the auricle, or the heart would pass into auricular fibrillation, and resume the normal rhythm after the effect of the drug passed off. In all these cases, little real benefit accrued to the

patient from the effect of the drug, save when there was a rapid action of the heart with marked slowing from the use of the drug, or when there was dropsy followed by an increased flow of urine. The mere statement of the patients that they felt better, was not taken as evidence unless there was clear evidence of some effect upon the rate or rhythm of the heart, and evidence that the improvement was not due to other agents, as rest.

There was one type of case in which the effect of the drug was at times phenomenal. These were cases where the heart failure was often extreme, with orthopnœa and dropsy. The heart was rapid and irregular, typical of the condition I had described at first as paralysis of the auricle, but which we now recognise as auricular fibrillation. As soon as a sufficiency of digitalis was given, the heart's rate speedily slowed down, it may be from 130 and over, to 60 or 70 per minute, and at once all signs of distress disappeared, and the patient would be able to lie down, and later go about his affairs. In a large number of cases I found this effect would result after taking one drachm of the tincture of digitalis per diem for five to seven days. This is the quantity that would produce partial heart block in some, or sickness and vomiting in others, in whom the heart was untouched by the drug. This reaction to digitalis led to a study of heart failure in these cases, and I found in a great number, that where the heart's rate was rapid, there was increasing sign of heart failure, which disappeared with the slowing of the rate, under the influence of digitalis. I then proceeded to find out the quantity that kept the heart at a moderate rate, and found that wherever a rapid heart was slowed down under the influence of the drug, by taking small quantities at intervals, a lasting effect

could be obtained which would keep the rate down and stave off attacks of heart failure, and the patient would be able to lead a useful life. This line of observation required the observation of individuals for many years.

I trust I have no exaggerated opinion of my ability to combat disease, for I am conscious of my helplessness when confronted with most diseases; but this I can say, that I have, time and again, seen patients suffering from orthopnœa, Cheyne-Stokes respiration, dropsy, and all the signs of extreme heart failure, and who were given up as hopeless by their medical attendant, speedily make a rapid recovery that has been maintained for years, by the skilful and intelligent employment of this drug.

The chief object I have in citing my observations on digitalis is, that they have brought to light the only rational way that remedies can be intelligently studied. They show that before the properties of a drug can be ascertained, it is absolutely necessary that the symptoms of the diseased state be recognised, and their significance understood. I found that the reaction to the drug depended on the nature of the heart lesion, the reaction varying with the nature of the disease. It will thus be seen how hopeless it is for physicians, ignorant of the different forms of irregular action, to recognise the peculiar reactions of digitalis, or the nature of the abnormal conditions that modify the action of the drug, or to expect to recognise from laboratory experiments on healthy animals the effects of drugs on diseased persons. Before this work was undertaken, and the mode of action of digitalis understood, an enormous amount of research had been carried out, both by physicians and experimental pharmacologists. The result was to leave the matter in a state of chaos. It was recognised that the drug

did good, but the knowledge of the kind of case that it benefited was wanting, so that every patient who had, or who was supposed to have, a cardiac affection, was given the drug. A long search into the literature revealed the fact that there was not on record a single case in which the effect of the drug on a human being was recognised and intelligently described; and even at this day the majority of investigators into the effects of remedies, have failed to appreciate the action of remedies on the diseased human body.

PART III.

CONSTRUCTIVE

INTRODUCTION

The citation of these experiences is intended to supply the data for the recognition of certain principles which may help in the investigation of the symptoms of disease. There are certain defects in our knowledge, the persistence of which is to be ascribed to the fact that the method to be employed for removing them has not been understood. There is a vast amount of very useful knowledge of symptoms, but the knowledge is not as fruitful as it might be, because much of it, being the result of personal experience, is of that vague indefinite kind which does not permit of a description which brings conviction to others. I have attempted to discover measures that might help to render this kind of knowledge precise and definite. I remember in the early days of my practice attending an elderly gentleman with dropsy from heart failure, and having a consultation with a skilled physician. He made a brief examination and gave a hopeful outlook, cheering the patient by saying he would get better. I was somewhat surprised at the prognosis, as the patient was evidently very ill, and I asked the physician how he came to that conclusion, and he replied that no one with a heart of this type dies during the first break-

down. I asked him why this was so, and he said he could not tell, but his experience had justified the statement. In later years I found he was quite right, and I also found out the reason after a prolonged inquiry into the different phases of heart failure.

In this section I direct attention to the principles and methods which guided me in making good some defects in my knowledge of symptoms. Many of these principles and methods are self-evident, and have no doubt been used for a long time, but they have not been employed in a systematic manner with a clear perception of their usefulness. It may be considered that my results are too meagre to justify the employment of the methods advocated. While I recognise that I have not got far, I would point out that a great part of my time has been taken up in devising methods and finding out principles—a laborious spade work which had to be done, if ever symptoms were to receive that meed of attention which advance in medicine demands. The additions I have made to medical knowledge, small though they be, could only have been made in this manner; but I have got an insight into the matter which makes me confident that it is only by the employment of such principles and methods that progress can be achieved.

CHAPTER I

PRINCIPLES OF RESEARCH

All investigations should be carried out in an orderly manner, and be guided by definite principles. So far this conception has been lacking in the investigation of the symptoms of disease, with the result that the knowledge, even of the most manifest symptoms, is imperfect, and their usefulness has consequently been greatly restricted. As a knowledge of symptoms is necessary to the study of disease, the progress of medicine is hampered, because there is no clear understanding how the full advantage of valuable discoveries should be taken.

If we take a method of examination, such as auscultation, we find little advance in the kind of knowledge it brings to light beyond what was found 50 years ago. This has not come through a lack of endeavour to extend its usefulness, but because these endeavours have been misdirected. They have taken the line of trying to improve the methods by modifying the instrument, and now, after the expenditure of much energy, no advance has been made, for auscultation to-day is found to be more accurate by the use of the original wooden stethoscope, or even by the direct application of the ear to the body, than by any of the mechanical devices on which so much labour has been spent.

The history of the use of the thermometer is prac-

tically the same, and the kind of knowledge that it reveals to-day is as restricted as it was 50 years ago, and the same criticism applies to nearly every method of clinical examination. Even a great discovery like the microbic origin of disease, threatens to have its possibilities restricted because of a lack of knowledge of how to employ it to the best advantage.

It may be contended that methods of examination like auscultation, and the taking of the temperature, are employed for definite purposes, and their usefulness is fully understood. This, indeed, is the attitude which leads experienced physicians to say that the limits of physical examination has been reached, and justifies them in calling upon the laboratory workers for their assistance in discovering new methods.

In carrying out researches of this kind, I gradually realised there were certain principles which guided me to the result at which I aimed. These at first were far from clear, but as time went on and as I attained a better insight into research, they became so clear that I was able to formulate them into definite laws or principles for my own guidance. In a sense there was nothing original in these laws, as in a confused sort of way they had been followed by every investigator, but their significance had not been realised, so that they were not used with a clear understanding of their import, and their neglect has led to that restriction of a great many valuable discoverics in methods and symptoms to which I have referred.

The Law of Progression.

There is one principle which should be placed in the forefront of all medical inquiries, viz., that the discovery of a fact should not be the end of the investigation, but should be used for the discovery of other facts—that every step forward should be but getting

an advantage for the taking of another step; what I may call the "Law of Progression."

The significance of this law has not been realised. It might be said that auscultation was an important discovery, and its employment in the detection of diseases of the valves of the heart has been carried out with meticulous care, so what more can be expected—its limit of usefulness has been reached. As a contrast, take the experiences described which led to the investigation of the jugular pulse. Others had studied the subject before, and, beyond recognising some of its features, left the matter as one of no practical importance. On the other hand, I used it as a stepping-stone for a further advance, and by its means the mechanism of irregular heart action was revealed. Irregularity of the heart was also investigated by other observers, who were content to leave the matter after they had found out the physiological basis on which the different irregularities were produced. Again I used the knowledge in other ways. One way was, to find out the action of digitalis on the human heart. Another way was to find out what happened to people who showed the different irregularities, and in watching these patients I found out the prognostic significance of the different forms of irregularity, a matter of the greatest practical importance. In the course of this inquiry I discovered the clinical features of the condition now called auricular fibrillation, but realising that the methods I employed did not permit of a complete knowledge of the condition, I sought additional knowledge by interesting others in the matter, so that the use of the electro-cardiograph and experiments on animals revealed its true nature. The recognition of the condition was also used as a means for studying the onset of heart failure, an investigation which, though still far from complete, has thrown new

and unexpected light upon a problem of the greatest importance in the practice of medicine.

The Law of Associated Phenomena.

Admitting the necessity for using one new fact as a means for the discovery of other facts, the question arises, how is this to be done? In the examination of patients whose health is impaired, there is always found a number of symptoms. In many cases one or more symptoms are predominant; the cause of the symptoms not being understood, the disease is sometimes described by its symptoms as albuminuria, angina pectoris, exophthalmic goitre. Besides these dominating symptoms there are others which call for recognition which, when found, sometimes throw a flood of light upon the diseased state, so that the observer is enabled to recognise features of real practical importance. This method of research is guided according to what I may call the "Law of Associated Phenomena," this law being based upon the fact that disease, in modifying the function of an organ or in impairing the health of the individual, produces a variety of phenomena, and the application of this law demands a search for the less prominent symptoms, and an inquiry into their nature. The immediate objects aimed at after detecting a symptom are (1) the discovery of the mechanism of its production, and (2) the determination of what bearing its cause has on the patient's future. These two objects give a definite quest, which can be obtained in many cases by the application of this law.

It was in order to demonstrate the usefulness of this law that I entered into so much detail in giving my experiences. A simple illustration of its application was shown by the detection of the cause of the mid-diastolic murmur. For 50 years this murmur had been recognised by clinicians, but no one had given the true

explanation of how it was produced. The application of the law of associated phenomena revealed its nature and showed how the usefulness of auscultation could be extended. Thus it was recognised that a certain force is necessary to produce a murmur. What force is there during the pause after the contraction of the ventricles till the contraction of the auricles? Under normal circumstances there is none beyond the rush of blood out of the auricle into the ventricle at the end of the ventricular systole. In mitral stenosis this gives rise to the diastolic murmur, which follows immediately after the second sound. But the mid-diastolic murmur is separated from both the first and second sounds (Fig. 27). Searching for associated phenomena, a tracing of the jugular pulse revealed that the murmur occurred at the same time as the auricular systole, which was separated by a pause from the ventricular systole. Not only was the cause of the murmur revealed, but the recognition of the delay of the ventricular systole after the auricular showed that the auriculo-ventricular bundle was impaired, so that now we recognise that a midiastolic murmur is not only an evidence of mitral stenosis, but a sign of partial heart block.

The chapter on mitral stenosis describing the changes in the murmurs and their significance is another illustration. A similar instance of the application of the law will be found in the discovery of the different forms of irregular heart action. Another illustration is afforded by the account of the investigation of visceral pain. This sensation has been the subject of investigation for ages, yet little progress was made until the application of this law, which resulted in the discovery of its association with the viscero-sensory and viscero-motor reflexes, and this gave a guide as to its mechanism. The application of this

law will ultimately lead to a further advance in our knowledge of this subject, when those who have the opportunity understand its value, in the sense that the various sensory phenomena will be closely co-related with the conditions discovered at operation and on the post-mortem table. So far, this investigation of pain is in its infancy, but the principles upon which it must be conducted are now being recognised, and it simply requires those who have the opportunity to apply them.

In the practice of medicine it is essential to know what bearing the cause of the symptom has on the patient's future. To the solution of this problem, the application of this law is necessary, and I will enter into some detail to prove this in the chapter on the assessment of the value of symptoms.

The application of this law is necessary in other fields of research. It is manifest that, in the study of disease of an organ, it is essential to recognise the efficiency or degree of inefficiency of the functions of the organ. As the modifications of the function can seldom be made out by the study of the organ itself, but are recognised by the reaction on other organs, it follows that this law of the associated phenomena has to be systematically applied in such an inquiry.

As the invasion of the body by disease produces a number of symptoms, the necessity for applying this law to detect the symptoms produced by the entrance of a microbe is evident. As the introduction into the body of an active drug produces a number of symptoms, the study of the action of remedies can only be properly performed by the application of this law. Before the drug is given, the various symptoms of disease must be recognised, so that the action of the drug in modifying these, or in producing new symptoms, should

be understood. This was illustrated by the description of the action of digitalis on the diseased heart.

In the next chapter the classification of symptoms will be dealt with, and the importance of this law in leading to an intelligent grouping of the symptoms will be demonstrated. As an outcome of this classification, it will be shown in a later chapter that a great step can be taken to recognise the principles underlying the production of symptoms; and with this better understanding of principles the study of disease, and the practice of medicine, become greatly simplified.

This law has been employed from time immemorial. It is by the associated symptoms we recognise many diseases, as the exanthemata and exophthalmic goitre. It has been constantly used in the discovery of focal lesions of the central nervous system. What I am urging is, that its importance should be recognised and applied systematically in the investigation of disease. I repeat that it is because it has not been thus systematically employed that so many valuable discoveries have been rendered sterile or their usefulness in practice greatly restricted, while the progress that should follow their discovery has been hampered.

CHAPTER II

THE CLASSIFICATION OF SYMPTOMS

Importance of classification.

All sciences pass through a stage in which the collection of facts is without any ordered arrangement. There comes a time when differentiation and classification is attempted. At first these attempts will be more or less empirical, being based upon superficial resemblances. As inquiry proceeds and knowledge increases, these will be found insufficient, and one will replace another till a classification based upon nature's laws will be discovered—such is the history of the evolution of such sciences as botany and chemistry.

However crude and imperfect the earlier classifications were, they served a useful purpose, in that, for a time, they gave a definite guide for further investigation, and the fact that their insufficiency was realised indicated a distinct advance. Indeed, the stage in evolution which a science has reached may be told by the character of the classification of the facts.

Phenomena in nature—whether pertaining to the universe or to disease of the human body—exhibit certain definite relations with one another. Where the relationship arises from the action of the processes of nature, a basis for classification will be found.

The recognition of this principle affords a guide for a scientific classification of disease. If we detect the features peculiar to one structure, which owe their presence not to some temporary or artificial circum-

stance, but to some factor inherent in nature, then we have a definite guide to search for like features in the constitution of other structures. Thus it is that the classification based on natural affinities has raised botany to a science, while the discovery of the atomic theory enabled a classification of elements to be made based upon a natural relationship, and this helped the advance of chemical science.

The need for a scientific classification of disease must be borne in upon everyone who gives the matter serious consideration. For any doctor who engages conscientiously in general practice to look at a modern book on general medicine, is for him to feel despair. The multiplication of diseases and the methods for detecting them increases at an alarming rate, so that it is hopeless for him to keep pace with them. This despair is not likely to be less when he reflects that with most of the complaints for which he is consulted, only specialists are supposed to deal efficiently. Medicine therefore, on the present lines, is tending to become more and more complex and chaotic.

From the study of the history of science it can be stated that a classification based upon the laws of nature tends to simplification, as these laws are few in number. Until these laws are discovered, additions to knowledge are haphazard, and reveal only isolated phenomena. Such "progress" leads to an accumulation of detail, and the subject becomes complex and confused. With these details arranged in groups based on natural affinities, the subject becomes clear and simple.

This would imply that if medicine is to become a science there is a need for a revision in our ideals, as well as in our methods. How is this to be accomplished? The aim should be to find the principles that underlie the production of phenomena. A search for

principles compels not only a recognition of phenomena, but an understanding of how they are produced, and what they signify.

At present the classification of disease is based upon no principle, and is no more than an assemblage of conditions grouped according to the organ affected, or the nature of the infection, or the most predominant symptom.

Before a classification of disease based upon sound principles can be arrived at, it is necessary to have a knowledge of the manifestations of disease. I do not deal with the classification of disease, as I do not possess the knowledge necessary for such an undertaking. Before this can be done, we must possess a fuller knowledge of the symptoms of disease, so that I deal only with a classification of symptoms.

Definition of terms.

Disease is made known to us only by the presence of certain manifestations which we call symptoms, or signs of disease. In using the terms symptoms and signs, I do not separate them as distinct entities. Attempts have been made to make a distinction, as limiting "signs" to manifestations detected by the physician, and "symptoms" to the sensations experienced by the patient. In practice, however, it is difficult to maintain such a distinction, as for instance, when in the course of examination the physician finds that the patient feels pain and winces when he presses on a portion of the external body which is hyperæsthetic. The term "sign" or "symptom" may be applied indifferently to such an observation. Probably if we were to be meticulously strict a distinction might be found, but I use the terms as interchangeable one with another, and with such terms as manifestations and phenomena.

Principles underlying a classification.

Such classifications as have been attempted in the past have not been satisfactory. Symptoms sometimes are described as separate entities, with little consideration of their relations to allied phenomena. Usually they are described in connection with the organ which produces, or is supposed to produce them. Occasionally an attempt is made to classify them into objective and subjective, but such a classification does not carry us far.

In searching for a basis I have adopted one which in my present state of knowledge fulfils certain essential requirements, inasmuch as it is based upon natural laws, and gives at the same time information of a kind that is necessary in practical medicine, while its application guides one to an efficient examination of the patient.

To understand on what basis a classification of disease should be made, we have to consider the manner in which symptoms are produced. Disease, where it impairs the health of the body, always produces a variety of phenomena or symptoms. Some of these may result from the disease itself, *causing a structural change in the tissues*, producing thus what is called a physical sign of disease. In every case of impaired health, the disease reacts upon the organs, *causing a disturbance in their function.* Such disturbances may be more evident than the signs actually indicative of the disease, and because of their prominence they may be mistaken for the disease itself. Amongst these disturbances of organs there is one class so distinctive and peculiar in its mechanism, that it can be separated into a group by itself, particularly as its function is in many cases protective. The symptoms of this class *arise from stimulation of the central nervous system*, and their appearance are often the first to direct atten-

tion to the fact that the individual is ill, and they also indicate the source of the trouble.

Taking the mechanism of their production as a basis, symptoms can thus be classified in three groups:

(1) Structural symptoms.
(2) Functional symptoms.
(3) Reflex symptoms.

(1) Structural Symptoms.

Before we can detect a change in an organ, we must have a knowledge of its position, its shape, and size, and other characteristics as revealed in the healthy body. A deviation from what we recognise as its appearance in health is called a physical sign, and physical diagnosis is the term applied to the detection of disease by the presence of these signs. It is necessary to hold clearly in mind what is revealed when we detect a physical sign, either by the unaided senses, or by means of the many mechanical means employed. In the main, a physical sign is due to an alteration in the structure of the organ; though in this class is included also such signs as the modification in the sounds and movements of the heart, and altered sensations. The detection of a physical sign gives little information beyond the fact that a change has taken place in an organ. There may from experience arise a knowledge that certain signs are associated with conditions of a definite nature bearing upon the health of the patient, as an enlarged heart is often associated with heart failure or a malignant tumour is accompanied by ill-health, but properly speaking, the signs of heart failure and impaired health are not revealed by the physical sign. It might seem to be a needless refinement to insist upon such a distinction, but for the purpose of keeping clearly in mind the limitations of knowledge revealed by a physical sign, such a distinction is necessary, for we find that in practice the detection of a

physical sign is often thought to convey information far beyond what it actually reveals. Thus for the last 100 years the detection of a murmur in the examination of the heart has led to the assumption that the heart was seriously affected, and we see to-day how this view misleads the profession.

This failure to appreciate the significance of a physical sign is found in connection with most diseases. The detection of an impaired percussion note, or of a shadow in the lungs revealed by the X-rays, is judged to be sufficient evidence for a prolonged course of treatment. Totally unnecessary fears are frequently raised by the findings in an X-ray examination, or by other instrumental methods.

The limited knowledge and the peculiar kind of knowledge revealed by a physical sign must always be kept in view, as the neglect of this misleads, particularly in the use of mechanical devices in the detection of symptoms, as I shall point out later. Another matter to be kept in view is that a physical sign due to some structural alteration in an organ, may be the result of a functional disturbance of another organ, as in the eye signs and circulatory disturbances in disease of the thyroid gland.

(2) Functional Symptoms.

The essential matter in the maintenance of health is the functional efficiency of the organs of the body. A structural modification may take place and leave the efficiency unimpaired. Thus it arises that after the detection of any physical sign, a careful inquiry must be made into the efficiency of the affected organ. The evidences of function are not always easy to detect, but it may be taken for granted that the disease of any organ will never be properly recognised until the function of the organ, and the part it plays in maintaining

the health of the body, is understood. While a certain amount of knowledge may be obtained by studying the activity of the organ during health, a far better understanding will be obtained by studying the modifications of functions. These modifications can only be recognised by detecting the manifestations they produce on the body—i.e., functional symptoms.

Functional symptoms are rarely detected from direct evidence in the organ affected but rather from the effects produced on other organs. This is seen in its simplest form in certain affections of the thyroid gland. The structural signs give no indication as to the serious or simple nature of the affection, this is only found out by the effect of the perverted activity of the gland upon the heart, eye, and nervous system, and in the increased oxidation of the tissues. Diminished functional activity, as in myxœdema, is shown also only by its effects on remote organs and tissues.

Even in an organ like the heart, whose condition and activity can be so easily studied, the information essential to a knowledge of its condition can only be made out by the reaction on other organs. The study of its various manifestations gives no idea of its functional efficiency, and this knowledge can only be acquired by observing how the circulation is maintained in other organs, as shown by dropsy or enlarged liver due to heart failure. Even in the early stages of heart weakness, the essential signs are brought about by a failure of the heart to supply sufficient blood to remote organs.

In affections of the kidney, the symptoms essential to a knowledge of the state of the kidney are not elucidated by an examination of the kidney, and even the elaborate chemical and microscopic examination of the urine fails to yield information so important as the presence or absence of changes in the heart and blood vessels.

It will be seen that not only do organs remote from the original disease exhibit the essential symptoms produced by depraved function, but organs thus affected in this secondary manner may show such signs from this cause that they form the principal feature in the picture of impaired health. Indeed it will be found that a great many diseases which are described as independent and distinct, are but the reaction to the depraved function of another organ. This is best seen in the response of the heart. Probably the depraved function of every organ reacts upon the heart, either through the nervous mechanism of the heart, or from its nutrition being impaired, as in anæmia.

The importance of recognising this class of symptoms as distinct, is because it gives a line of investigation in each case. Already we know of many symptoms produced by different organs which are associated in their appearance, and we recognise certain groups of symptoms as being due to the altered or impaired function of an organ, so that the appearance of one of a group leads to a search for others. Moreover, the increase in our knowledge of the mechanism of the individual symptom will inevitably lead to a better understanding of the organ whose depraved function is the cause of the disturbance, and thus contribute largely to that very necessary branch of knowledge—the pathology of function.

It might seem unnecessary to point out how important this view is from the standpoint of treatment. It is manifest that if such an organ as the heart is disturbed by the depraved functions of other organs, any treatment which is devoted to the heart will be useless. Yet this view needs emphasising, for to my knowledge, large numbers of individuals are submitted to prolonged treatment for cardiac symptoms in which the organ is only secondarily affected. Indeed, so impor-

tant is this aspect of the matter that the question should arise in connection with every disease, whether the symptoms are not manifestations of a disease provoked by some other organ. The importance of this point of view will be more fully considered later.

(3) Reflex Symptoms.

There are a great many diseases in which we fail to detect any structural sign or any functional impairment, yet we can recognise the disease with great accuracy. The signs by which we are able to do this result from the irritation of a limited portion of the central nervous system by a peculiar form of stimulation produced by the diseased organ. Thus in certain cases of appendicitis, the only signs may be a sense of uneasiness or pain in the right iliac region, an excessive sensitiveness of the skin and deeper structure of the abdominal wall to pinching and pressure, and a rigidity of a portion of the muscle wall in this region. There may also be present a frequency of micturition. All these phenomena may speedily disappear after the removal of an inflamed appendix. This has happened so frequently that we now assume with good reason that the foregoing signs are evidences of appendicitis, though no direct evidence from the appendix either structural or functional can be obtained. How then are these symptoms produced? It has been assumed by the careless observer that the pain on pressure was evoked by the pressure being made directly on the appendix or inflamed peritoneum, and the contracted muscle is often taken for the greatly enlarged appendix. But a careful examination will reveal the fact that the skin itself may be the seat of the pain on pressure, and that the muscle is actually in a state of tonic contraction, while on operation the appendix may be found behind the cæcum at some distance from the abdominal wall, and there may be no peritonitis.

Phenomena of exactly the same kind can be found in other diseases such as gastric ulcer, and gall stone disease, while no direct evidences of these diseases are available, and there is an absence of structural and functional symptoms.

We are therefore justified in making a group whose phenomena arise in a way different from the two preceding classes of symptoms.

The mechanism of Reflex Symptoms.

When we group all the reflex symptoms together with their cause remote from them, we are driven to seek for a solution through the nervous system. Such symptoms as a hyperalgesia of the skin with a contraction of the muscles, indicate the involvement of sensory and motor nerves supplied to the external body wall and the stimulus producing them can only arise from some place where the nerve centres lie close together. Such a centre is found in the spinal cord. In appendicitis, for instance, the nerves supplying the skin and muscles of the affected region have their origin in cells in the section of the spinal cord from which arise the 11th and 12th dorsal nerves. Here also is situated the centre for micturition. We can therefore say that the symptoms present in such a case are due to a lesion or rather a persistent irritability of the spinal cord—*an irritable focus of the spinal cord.*

We know so little of the innervation of limited portions of the intestinal tract, that we cannot as yet tell to what portions the sympathetic nerves are distributed. But when this method of observation is understood and applied, we will obtain a more exact knowledge of the distribution of the sympathetic or autonomic nerves. There is some justification for assuming from the foregoing reasoning that the appendix when it is diseased elicits a stimulus of such a nature as to affect the nerve

endings in the appendix, which stimulus is conveyed by its afferent nerves to that portion of the spinal cord whose nerves supply the skin and the muscles in the right iliac region. From such observations we can gather the source of the nerve supply to the appendix, and the mechanism by which the phenomena of pain, hyperalgesia and muscular contraction arises. This leads to a consideration of the principles involved in the production of this class of symptoms, and to that end I put forward the following hypothesis which seems satisfactorily to explain a good many other phenomena at present somewhat obscure.

The nature of Reflex Symptoms.

A great field of medicine still to be explored is the purpose of the functions of the organs. We all know that every organ has a purpose, but it is not clear what that purpose is, except in the most evident of organs such as the stomach and heart. In many organs we have a partial knowledge of what they are for, but even in those where the function seems plainest and most evident, we may be far from realising the full extent of their purpose. If we take the skin and muscles of the external body wall, it might seem that their function and purpose is obvious, and they had been sufficiently studied. Nevertheless, there are peculiarities in their functions which have received so little consideration, that some of their functions have never been appreciated. The same remarks apply to certain functions of the nervous system, particularly the part they play in relation to the diseases of the viscera.

To understand the part played by the skin, muscles, and nerves in diseases with which they have no direct connection, it may help, if the development of the body with the relation of the viscera to the external body wall is recognised. In the early development of animal

life a digestive cavity is first evolved, then, later, a rudimentary circulatory system appears. To this is added a rudimentary respiratory system. To co-ordinate these different systems, a nervous communication is developed. The various organs are protected first by an insensitive and, it may be, a somewhat unyielding outer covering. As development proceeds this outer covering becomes modified in such a manner as to provide for both protection and movement, hence arises the sensori-muscular system. Protection is secured in a two-fold manner, first, by rendering the outer covering sensitive and uniting it with muscles by means of a reflex nervous system. Certain stimuli not only cause pain but excite the muscles to contract, so that the organism is removed from the offending neighbourhood, or there is interposed between the viscera and the offending agent a hard resistant muscle. Second, by uniting the nerves from the viscera with sensory and motor nerves of the cerebro-spinal system, so that the muscles of the external body wall react to a stimulus from the viscera. From this point of view the primitive nervous system corresponds with some part of the sympathetic, while the more recently developed sensori-motor corresponds with the cerebro-spinal nervous system. This finds support in view of the fact that pain is elicited only by stimulation of some part of the cerebro-spinal nervous system, as distinct from the sympathetic or autonomic system, while such stimuli as produce pain and other sensations in the skin and structures of the external body wall are inadequate to produce these sensations when applied to tissues supplied by the sympathetic nerves (page 75). When pain does arise from the viscera it does so by calling into play the cerebro-spinal system of sensory nerves, and the pain is then referred to regions supplied by the cerebro-spinal system of nerves.

From this point of view it will be found that some of the most striking symptoms in disease are produced by reflexes, sensory, motor and organic.

Reflex phenomena in renal or gall-stone colic.

The character of the phenomena evoked by the reflex group of symptoms is well seen in attacks of renal or biliary colic. It seems reasonable to suppose that the immediate cause of the stimulus in these cases is the contraction of non-striped muscle due to a calculus or gall stone stimulating the muscle to violent contraction—such an inference is drawn from the study of pain in peristaltic contraction of the bowel as described on page 71 and from the study of uterine pain. In renal colic uneasiness is first produced in a definite region of the body. Gradually the uneasiness becomes distressful and passes into pain which reaches an agonising degree. The skin in the region of the pain becomes hyperalgesic—the hyperalgesia persisting after the attack of pain subsides, sometimes being very acute. The broad muscles of the abdominal wall may contract and remain hard and board-like for a considerable time. The pain slowly waxes and wanes in wave-like intensity and during the height of the wave nausea and vomiting may be repeatedly set up. The heart becomes weaker in its action, and the pulse soft and compressible. The exhaustion of the heart may proceed so far that that peculiar irregularity—the pulsus alternans may be produced. This irregularity is usually associated with, or is the effect of, great exhaustion of the heart muscle. The face becomes pale and drawn, and beads of perspiration break out on the forehead. Partial or complete loss of consciousness may result during the height of the pain, and the collapse be so great that it resembles what occurs in surgical shock.

It will be noted that all these symptoms are produced by a reflex stimulation of the central nervous system.

The application of this classification of symptoms will be shown in subsequent chapters.

CHAPTER III

THE ASSESSMENT OF THE VALUE OF SYMPTOMS

The neglect of prognosis.

In the employment of our knowledge of symptoms in practice, it is necessary to know what bearing the cause of the symptoms has on the patient's future. This matter is usually described under the head of "Prognosis," and its importance is so self-evident that every clinical observer deals with it when describing any form of disease. Indeed, so universal is the recognition of its importance that it is assumed that the subject must have been as efficiently dealt with as is necessary, and the matter is supposed to be so simple that anyone can employ it in practice. So far is this from being the case, that there is nowhere, to my knowledge, a description of the subject which reveals its true nature, and there is no indication as to how it should be studied and efficiently applied.

As a matter of fact, the advances that have been made in medicine have been so much concerned with other aspects of disease, that the study of prognosis has been almost entirely neglected. Not only the methods to be pursued, but the individuals qualified for its pursuit have never been recognised. In place of being easy and simple it is one of the most difficult subjects, and requires for its pursuit a profound knowledge of the symptoms of disease.

Importance of the science of prognosis.

Medicine includes within its compass a number of branches which are called sciences, as physiology and

bacteriology, but none will be found better to deserve the name of science than that of prognosis when its significance is fully realised. The attainment of its knowledge puts the coping stone on the whole system of medicine, and all the other branches are contributory to it. It demands a knowledge of the cause of a disease and of its progress from start to finish, of the reactions of organs and tissues the one upon the other, and of the effects of remedial measures in modifying the progress of disease. This is a kind of knowledge which is essential in the practice of medicine and which no academic investigator can ever acquire. If medicine is ever to reach the status of a science, it can only do so when a fuller knowledge of this subject is attained, and this knowledge can only be acquired by adopting special methods of inquiry.

Prognosis has hitherto been chiefly studied in advanced disease, and a considerable amount of reliable information has been accumulated. But there is a wide field requiring further investigation, particularly in regard to symptoms, before much damage is done.

When a patient consults a doctor, the question put directly by the patient or implied, is what is to be the outcome of my complaint? This infers that the patient has not only a confidence in the doctor's power of diagnosis, but also believes he possesses the knowledge which will enable him to foretell the progress of the disease if left unchecked.

Not only does the patient credit the doctor with being able to tell the outcome of his complaint, but he expects the doctor to know the remedy that will check the disease and cure it. The doctor, therefore, has a great need for this particular kind of knowledge, for it is not only a guide in reassuring the patient or of warning him, but it affords the information by which the patient's future may be regulated. He has to

know what bearing the patient's mode of life, his work, and his food may have on the disease. He has to know whether the disease is amenable to treatment or needs treatment. In using his remedies, the doctor has to know how they act—not only in health but also how their action is modified by the disease. It will be seen that a knowledge that enables a sound prognosis to be made, includes many branches of medicine. Its investigation requires not only a good knowledge of the symptoms of disease, but opportunities for studying disease of a kind that has hitherto not been recognised as necessary.

The method by which symptoms may be valued.

The main steps for the study of prognosis are, first, to differentiate clearly the symptoms, so that each symptom may be separated from others which it resembles; second, to follow up individual cases in which the diseased condition persists; and third, to note all associated symptoms present at the time of discovery, and as they appear in the progress of the case.

(1) *Differentiation.*—In most people who are ill some sign is usually more conspicuous than others, and it is often because of this that the patient consults the doctor and for which the doctor applies his treatment. It is necessary clearly to separate this from other signs it resembles. This is so apparent that it seems unnecessary to dwell upon it. But it is probably because it is so self-evident that it has been neglected—everyone supposing that it was easy to do. I have already shown, for instance, that irregular actions of the heart are of different kinds, and have a varied significance. Some are physiological, occurring in healthy subjects, others are so slightly pathological as to be of themselves of no significance, while others indicate a change in the heart action so profound as to be associated with

various degrees of gravity. Until recent years practically no attempt was made to separate them into categories based upon sound physiological principles, and it is only since this has been done that their prognostic significance has come clearly out.

Cardiac murmurs have indeed been studied with meticulous care, and an importance attached to a great many of them which was unjustified. It is a matter now commonly recognised that a grave significance has been attached to many murmurs which was not the result of careful observation. It has long been recognised that murmurs do vary in significance, but no efficient steps had been taken to separate the innocent from the serious with that precision which could be used for instruction.

(2) *The prolonged observation of individual cases.*—I have shown in the section on personal experiences the steps I took to assess the value of symptoms by watching individuals for years. Others have attempted to do the same thing in other ways. The most common in use is to study different patients at the various stages. This is the method pursued by those who see a large number of patients, but who do not have the opportunity of watching individual cases for the whole period of a protracted illness, such as hospital physicians and consultants and other doctors who specialise in diseases of particular organs. It is the only method open to them by their opportunities. It has not yet been realised how limited is the amount of information obtained in this way, and that often of a misleading kind.

The passing of one phase of disease into another is as a rule so gradual and imperceptible that the subtle changes are often undetected and many essential features are missed. Mitral stenosis is a disease which every physician thinks he knows thoroughly, and I

have purposely given a detailed account of my inquiries into the subject to show the importance of recognising the subtle changes that go on and how they afford information of considerable value.

Another method sometimes adopted is the statistical, the most fallacious of methods. Here the most prominent symptom is taken—say, a cardiac murmur. A hundred cases are collected, with the ages at which the individuals die, and an average is taken which is supposed to give a fair idea of the danger to life. The reason that this method is fallacious is that the cause of the prominent symptom is rarely the cause of death. A patient never dies of the conditions which produce murmurs, for instance, though he may die of exhaustion of the heart muscle, towards which exhaustion the cause of the murmur may or may not have contributed.

The only way in which a truly scientific prognosis can ever be obtained is by watching the progress of the disease from its inception till its termination. It will at once be realised that such a procedure is almost beyond human power. Perfection cannot be obtained in this world, but an attempt to attain it adds greatly to human knowledge, and in this matter a persistent attempt to find out what happens to people with long standing complaints will reveal an enormous amount of information that will materially help in the advance of medical knowledge and greatly assist in putting prognosis on a sound basis.

The work can only be done by the individual who has the opportunity, and that is the general practitioner. I have already dwelt on his opportunities for seeing disease during its whole history, and once this conception is recognised by teachers, they will be able to impress upon the student the defects in our knowledge, and how the opportunity for remedying these defects lies in the hands of the general practitioner.

(3) *The Law of Associated Phenomena.*—If the significance of the classification of symptoms (page 187) be fully realised, it will be seen that no attempt at a prognosis should be made until a search has been made for all the associated phenomena. I have already pointed out that when disease impaired the health a variety of symptoms was produced. As a rule in any individual affected by disease, one or two symptoms are dominant. If these belong to the class of structural symptoms, they do not give sufficient evidence to justify a prognosis. The search should be made for evidences of functional impairment, or to see if the symptom is not a manifestation arising from the disturbed function of some other organ. In the description of the classification of symptoms the nature of the information afforded by each group was given. In applying this classification in practice, the conclusion can be drawn that so long as there is present but one abnormal physical sign no serious prognosis should ever be given. The reason for this is seen in the cases of a heart affection, for, if there is any serious affection of the heart, signs of cardiac inefficiency are always present. I have not studied the histories of individual cases in a sufficiently large number with disease of other organs, but so far as my studies have gone, it supports the principle that a grave prognosis should never be based on one sign alone. It is therefore necessary in every case to search for associated phenomena. It is only when these are perceived or their absence demonstrated that we can have the facts on which the value of any given symptom can be assessed.

CHAPTER IV

THE ESTIMATION OF THE FUNCTIONAL EFFICIENCY OF AN ORGAN

In endeavouring to understand the nature of my patients' ailments, I naturally speculated much in a crude and sometimes illogical way. One line of speculation was to find out what were the signs of the inefficient functions of an organ. The kidney arrested my attention, as I had many cases of albuminuria, and I learnt how to examine the urine chemically and microscopically, but I made little progress in understanding the significance of the abnormalities or supposed abnormalities I detected. When, however, I sought for evidences of a reaction on other organs of the body, I found signs which seemed to help to distinguish albuminurias of different kinds. I could draw a distinction between cases who showed varying degrees of arterial change and cardiac hypertrophy, and cases who showed no alteration in the circulatory system. These are matters of common knowledge, but it is incumbent that a relation should be established between these clinical signs and any abnormality detected in the urine. Although I was able to watch a number of people from the beginning of kidney trouble (as after scarlet fever), for many years, my results were too patchy and indefinite to be of much value, but had I been able to give as much time to this line of investigation as I gave to circulatory problems, I am confident a good deal of light would have been thrown upon the subject. Laboratory methods alone will never solve

the problem, as Cushny has shown in his review of the whole subject of kidney function, and I agree with him when he states that—"It seems necessary to study the results (of laboratory methods) in relation to the clinical changes and prognosis"—"Clinical observers should accumulate a sufficient body of statistical evidence, averting their eyes meanwhile alike from the physiological laboratory and the post mortem room."

The functional efficiency of the heart.

The study of the murmurs and modified sounds of the heart with the records I had taken of the movements of the heart, the arteries, the veins and the liver, with the study and differentiation of the abnormal rhythms as can be inferred from the tracings given in a previous chapter had given me a fair knowledge of the chief physical signs. The next step was to find out the prognostic significance of these signs. I found this could not be satisfactorily pursued until I had a knowledge of the functional efficiency of the heart, and more particularly of the signs which indicated an impairment of its efficiency.

On account of its accessibility to examination, and the ease with which its activities can be studied, the heart offers a very suitable organ for the purpose of studying its functional efficiency. In a manner this has been done from time immemorial, for the taking of the pulse has that object in view. But though the importance of the subject may have been realised, and many attempts made, both by clinicians and physiologists, to find it out, it was not understood how the inquiry could be pursued. Attempts have been made to acquire the knowledge by experiments on animals and the introduction of every new instrument during the last 100 years from the introduction of the stethoscope to the latest laboratory contrivance, have each been used to solve the problem.

The failure of these attempts was due to the fact that the only way this object could be achieved was not understood. The recognition of impaired efficiency can be attained only by using those methods that are peculiar to clinical medicine, i.e., the careful differentiation of symptoms with an appreciation of their significance and the watching of individual cases with varying degrees of impairment for a sufficient length of time.

But even when it is realised that such a subject has to be studied along such lines, it is difficult to see what course to take. The methods suitable to perceive the efficiency of one organ are different from that required for other organs, though the general principles may be the same. It was only after five or six years of persistent attempts to find a method, that I gradually realised how it could be done. I had spent much time on the methods usually employed for the purpose—carefully noting the character of the sounds of the heart and the various murmurs and how they were affected by effort, or by counting the heart beats before and after effort, noting the time it took for the rate to become normal, or by observing the amount of effort required to produce breathlessness and so forth. The provisional classification of patients based upon these observations I found, in watching them in after years, to be of little value, for some of those that I had deemed to have serious defects in their hearts turned out to be able to live energetic lives with no sign of limitation.

A conception gradually became apparent which seemed to offer a reasonable line of inquiry. I am not sure exactly when it arose, but one incident impressed itself upon me. In 1891 I had under my care a man of 58 years of age, who suffered severely from pain in the chest and left arm on moderate exertion. As time went on this pain could be so easily provoked

that he could walk only a short distance. Repeated examinations revealed no physical sign of disease, and when resting he felt quite well. One day he fell down from his seat and died at once. At the post mortem examination the heart was not increased in size, but the left ventricle was ruptured and the pericardium was full of blood. The wall around the hole was so atrophied that the muscle had disappeared over a space about one inch in diameter. This local atrophy was evidently due to a blocking of a branch of the coronary artery which was very atheromatous.

On reflecting on these circumstances, it was evident the coronary artery had been diseased for a long time and from this cause the muscle had been damaged. The damaged heart could maintain an efficient circulation when the body was at rest, but that on effort, when it had to act with more energy, it became speedily exhausted. The great pain was an expression of this exhaustion, and an imperative call for cessation of effort. From this experience it appeared that while the body was at rest a damaged heart may maintain an efficient circulation, but when the heart was called upon to do more work, the attempt to do this speedily exhausted the heart muscle.

The response to effort.

In all hearts there is a reserve of strength only called upon by effort, and on reflection it seemed likely that the first sign of the heart's strength being impaired would be shown by a premature exhaustion of this reserve force. How was the limitation of the reserve strength to be recognised? The attempts to get this knowledge from mechanical devices or laboratory methods had failed. It occurred to me to study the sensations produced by effort when pushed to the point of exhaustion on all sorts and conditions of people.

There speedily came to light certain phenomena which gave a line for inquiry.

At first the sensations produced by a failing heart seemed so indefinite that no clear conception of their nature or mode of production could be acquired. As the inquiry proceeded and a large number of patients examined a persistent attempt was made to have each sensation clearly differentiated. It was found that distressful sensations arose when effort was made beyond a certain limit in people with healthy hearts as well as in people with damaged hearts. In people with healthy hearts the distressful sensation was one of breathlessness as a rule, though in a few people there was a sense of constriction across the chest which almost amounted to pain. In people with damaged hearts, when heart failure set in, the symptoms were the same but produced by a slighter effort. This was best seen in people getting on in years in whom nothing could be detected amiss with the heart, and the only evidence of impairment was this limitation of the field of response to effort.

While breathlessness, occurring after an amount of exertion the individual was wont to undertake with comfort, was the most common sign, in some the tightness and oppression across the chest were the chief signs provoked, accompanied, in some people, by pain which in a few became so severe as to present the symptoms which are recognised as angina pectoris.

These two kinds of sensation are those which give the truest estimate of the heart's efficiency; the one breathlessness, belongs to the functional group of symptoms and arises from the heart failing to supply the centre for respiration in the brain with sufficient blood, the other oppression of the chest and pain, belong to the reflex group and arises from a stimulus being produced by the exhausted heart muscle, and

sent to the cells of the sensory nerves in the central nervous system.

Breathlessness.

The distinction between the signs of functional inefficiency (breathlessness) and of a cardiac reflex (pain and oppression) was based on the study of what happens in healthy people and in others who suffered from different forms of heart affections. Breathlessness, when it arises from cardiac inefficiency, is always found to be associated with a diminished output of blood, or rather, an output insufficient to supply the respiratory centre. Physical effort under normal circumstances calls for an increased supply of blood to the active parts. This is met by an increase in rate of the heart beat and a dilatation of the vessels supplying the active parts. It is unnecessary to dwell upon the fact that there is a limit even in health to the response of the heart to effort, that limit being shown by the distress in breathing.

This sign of cardiac inefficiency can be demonstrated to be due to the deficient supply of blood, by the study of certain morbid states. In most cases of auricular fibrillation the response to effort is accompanied by such an increase of the heart rate that many of the beats are ineffective and little or no blood may reach the periphery, and breathlessness is the most clamant symptom in the majority of these cases. In complete heart block, the rate of the ventricle beat is steadily about 30 per minute and it does not increase in response to effort. Many individuals suffering from this condition, if they hurry in their walk, are pulled up by breathlessness. A few may feel other sensations, as a swaying feeling as if they would fall, or their feet feel heavy as if loaded. These sensations are manifestly due to a deficient supply of blood to the brain and feet. There

are rarely purely cardiac signs such as pain, and I have never seen angina pectoris in a case of complete heart block, though the pathological changes in the two conditions have often a good deal in common.

Another instance when the heart's rate is not influenced by effort is in auricular flutter. Here the auricles usually beat about 300 per minute and the ventricles respond in many cases only to every second beat, the ventricular rate being about 150. Effort has no effect, as a rule, on either auricular or ventricular rate, but it provokes the sensation of breathlessness and not of pain. From these observations it is seen if in response to effort the output of the heart is diminished or not increased, breathlessness is readily induced.

Pain.

The reason for describing pain as an expression of exhaustion of the heart muscle arises from the consideration already given of the production of the viscero-sensory reflex (page 74). In the case described on page 155 the deduction was drawn from the symptoms during life, co-related with the state of the heart found post mortem. A series of other cases afforded similar evidence. Moreover, a consideration of what happens to any muscle when forced to work with an insufficient supply of blood, affords confirmatory evidence. Thus in cases with degeneration of the arteries of the leg, or where the femoral artery had been blocked, effort beyond a limited extent resulted in pain in the legs of such an agonising kind that the individuals were compelled to stop.

The inquiry led to an investigation of all sorts of individuals who suffered pain produced by effort, with a careful search for associated phenomena. In many cases, pain of varying degrees of severity was found, and on watching large numbers for years a certain

amount of knowledge was obtained, by which one could, with fair certainty, recognise the kind of damage and its extent. In many people the heart was not affected with actual disease, but the muscle was weakened from others causes which had impaired the health, as toxæmias, anæmias, impaired nutrition with overwork. In these there was much diversity in the character of the reflex symptoms, as the degree of pain, the circumstances provoking it, the extent of the hyperalgesia of the skin, etc.

Pain and breathlessness in the same individual.

This distinction between pain as a peculiar cardiac manifestation and breathlessness as a result of an inefficient output, is seen in certain cases of angina pectoris which ultimately develop auricular fibrillation. In the study of a few thousand cases of auricular fibrillation, I was struck by the absence of angina pectoris amongst them. Dull pain and aching and hyperalgesia of the left chest wall was not infrequent, but attacks of angina pectoris, even in a mild degree, in response to effort, did not seem to occur. As time went on patients who suffered from angina pectoris, and who were unable to walk far because of the pain, developed auricular fibrillation, and then ceased to have attacks of pain, being pulled up much sooner because of breathlessness.

The sense of exhaustion.

In the systematic inquiry into the nature of the sensation produced by effort, other sensations were recognised, the most frequent being a sense of exhaustion or fatigue, a feeling which causes a great desire to sit or lie down. In many cases it speedily passes off when this can be done. If forced to carry on some people become faint and lose consciousness.

There are many varieties of this sensation produced in different ways besides effort, as in standing in a hot room. It is assumed in many cases to be cardiac in origin, but it seems to be due to some vagal or vaso-motor disturbance inducing a cerebral anæmia, sometimes the result of a dilatation of the peripheral vessels, the large abdominal vessels, and the vessels of the skin, especially when the body is rendered warm by exertion and heavy clothing. As many of the people who suffer from this are debilitated from other affections, the heart is often weak from the same causes, but the sensation is not, properly speaking, a cardiac one.

Summary.

By following this line of observation, noting carefully all the associated phenomena occurring in individuals leading strenuous lives, and others who showed a progressive weakness of the heart, so that all phases of heart failure were observed, and having the hearts of those who died examined by skilled pathologists, certain deductions were drawn, of a simple kind, which became of the greatest service when applied in practice. Briefly these were :—

The heart's efficiency can be ascertained by recognising the manner in which it responds to effort.

The first sign of heart failure is shown by a sensation of distress on the individual undertaking some effort he was accustomed to perform in comfort.

The chief sensations of distress produced by the exhaustion of the heart are breathlessness or a sense of constriction across the chest or pain.

These results were obtained from individuals able to be up and about. Another series of observations were carried out to find the signs in people who are bed-ridden, and in cases of acute affections. The evidences

thus obtained, though different in character, were of the same kind and governed by the same principles. The value of these results will be demonstrated in the chapter on Simplification of Medicine.

CHAPTER V

THE SIMPLIFICATION OF MEDICINE

The urgent need for a simplification of medical knowledge.

THERE is one question which should call urgently for consideration by every one who is interested in medicine, and that is, can medicine not be simplified and its principles clearly understood so that the practice of medicine and research could be facilitated? It is manifest that its practice and investigation is hampered by the enormous accretion of details and methods.

If the statement that the better understanding of a science tends to its easier comprehension be true, what can be said for the stage which medicine has now reached, and the manner in which it is being pursued? It has become so complex that it requires a large and ever increasing number of individuals to undertake its teaching, and for the examination of a single patient a number of specialists are necessary. Its methods are so numerous that it needs special training for one individual to acquire a knowledge of but a few, while the phenomena which are revealed are so many and so varied in character that even the specialists who discover them are unable to understand their full significance. The enormous increase of details is far from having reached its limit, for the cry is for a further extension of the conditions which have brought medicine to this pass, and the schools and hospitals equipped with only a small number of special methods are yearn-

ing for a greater number so that they may equal their more favoured rivals.

The lack of knowledge of fundamental principles hampers the application in practice of any discovery. In place of realising this and setting about remedying such a glaring defect, the present attempts to advance medicine tend to aggravate the condition. Many members of the medical profession are wholeheartedly desirous of doing their best for their patients, and according to their lights they spare no effort to give them the advantages of the most recent addition to knowledge. As I write this, there comes to hand a medical journal, and in glancing over its pages I come upon this paragraph:—

"The New York Diagnostic Society, which was founded a year ago, intends to establish a hospital for diagnosis in New York. The building, to consist of six stories and a basement, will be provided with the most modern equipment for diagnostic investigations and tests. The institution is to be self-supporting. The cost of the site and building will be £50,000."

Our government is understood to be deeply concerned with the health of the people, and are contemplating taking steps to improve medical attendance. They are being urged to adopt some such method as that just quoted. It cannot be too strongly insisted, that all such methods deal only with disease after it has damaged the tissues, and that most of the methods employed in these investigations fail to bring to light the origin of the signs or their significance—matters which are essential to the intelligent and rational practice of medicine.

The accumulation of detail which results from these special methods without orderly arrangement marks a stage in the evolution of medicine as a science. It is reasonable to expect that a time will come when a

better knowledge of the principles governing disease processes will lead to a great simplification of the subject. Such, at all events, is the view that I take, and consider it imperative that the whole subject of medicine should be reviewed in order that the chaos of the present stage should be clearly recognised, and measures taken to guide medicine into channels that will tend to its clearer comprehension.

One of the objects in writing this book is to show that medicine can be made more simple in its practice and at the same time rendered more efficient. This does not come about through a superficial method of inquiry but is the result of a long investigation into the nature and significance of symptoms. One outcome of this investigation shows that the thorough knowledge of symptoms enables the teacher to make the subject easier of comprehension to his pupils. One can easily understand how bewildered the most versatile of students must be after a session studying the subjects enumerated on pages 35 to 39. If the idea I am putting forth here is understood, it will be found that the employment of mechanical devices as a rule represents a stage in the evolution of medicine, a necessary stage no doubt, in most cases, but still a stage, and a crude and elementary one.

What I propose to do now is to set forth an example of the simplification of medicine as applied to one organ—the heart, trusting to demonstrate how the matters essential to medicine can be expressed in a simple way, and by the application of certain principles, render practice at the same time easier, and more rational. What has been done in the study of heart affections can also be done with other organs, if the same line of investigation be pursued. This is a big claim to make, and I would not be justified in making it if I were not in a position to demonstrate that it has been done.

Simplification of Methods.

The recognition that the radial pulse was due to the systole of the left ventricle was brought about by the use of instruments, but now we do not need these instruments to give us this information—the stage for the use of such instrument for this purpose has been passed. The employment of accurately recording instruments revealed the mechanism by which irregularities of the heart was produced, so that the majority can be classified on a physiological basis. When thus clearly differentiated, instead of a confused number of different kinds, the varieties are found to be few and easily identified. If the recognition and differentiation of these irregularities had to depend on mechanical means, it was manifest that the good derived would be greatly restricted. By searching for and finding associated phenomena other evidences were obtained, with the result that most of these different irregularities can now be recognised by the unaided senses, so that students can easily be taught to detect the vast majority without any instrumental device. Here again instrumental methods, having served their purpose, were discarded in practice, though retained for investigation and demonstration purposes.

How the significance of abnormal signs can be rendered easy of comprehension.

After detecting a physical sign such as modified sounds of the heart, it is essential to know what happens to the patients who show these abnormalities. This consideration led to the inquiry to assess the value of symptoms, where it was shown that there is an intimate relation between symptoms, and that no physical sign should be valued by itself, its association with other symptoms has always to be discovered. Hitherto the great number of symp-

toms having no clear principle indicating their relation rendered the subject confused. The classification of symptoms into three groups simplifies the interpretation of symptoms, so that a recognition of the class to which they belong gives a guide to the nature of the symptoms and thus leads to an inquiry which should result in indicating the grounds for a rational diagnosis and treatment. For instance, hitherto an abnormal sign revealed by an instrument has been often looked upon as a sign obscurely indicating danger and calling for treatment. The classification I have given shows that all the signs revealed by instrumental methods belong to the structural group, and they give no warrant for a prognosis or a line of treatment, but require that other signs must be looked for, which indicate the functional efficiency or inefficiency of the organ.

The estimation of an organ's efficiency.

With this knowledge of symptoms, one was able to investigate the functional efficiency of an organ, or rather its degree of inefficiency. In health there is a harmonious working of all the organs, so that it is not possible to tell the part each one plays. When one begins to fail or its function is disturbed, then we get evidences which we can often assign to the organ at fault. The recognition of the signs of inefficiency of the heart gave a clear guide in applying the knowledge of symptoms in practice and simplified the subject.

The significance of every abnormal sign is determined by its relation to heart failure.

In every case of an affection of the heart, grave as well as innocent, the matter resolves itself into the question of heart failure. In every case the question is "Do these signs indicate heart failure or do they foreshadow its occurrence?"

This question, being realised, led to the inquiry into the signs of heart failure summarised in the last chapter, and the result of the inquiry can be summed up in the following propositions :—

(1) Heart failure is due to the heart muscle being unable to maintain an efficient circulation.

(2) The first step in heart failure is a limitation of the heart to respond to effort.

(3) The symptoms by which the early stages of heart failure can be recognised are sensations of distress produced in making an effort which the individual had been accustomed to make in comfort.

The significance of the patient's sensations.

When the seemingly confused and valueless sensations of distress are analysed with an intelligent understanding of their significance, they are found to be of the simple kind, chiefly breathlessness and pain.

By the intelligent interrogation of patients, with a due appreciation and understanding of their sensations, a knowledge of the heart's efficiency in the vast majority of patients can be got in a few minutes, more reliable and instructive than an examination made by a series of specialists employing the most elaborate mechanical means.

The importance of understanding what heart failure is.

It is not necessary to labour the point that the main consideration in organic disease resolves itself into the question whether or not the functions of the organ is interfered with, and in the case of the heart, the essential question is that of heart failure.

That statement is so simple and so obvious that it might be thought not to be worth while commenting on, and it is assumed that all about heart failure is so

thoroughly known that authors do not find it necessary to explain what is meant by heart failure. Some, indeed, described the "cardinal symptoms" of heart failure as dropsy, enlarged liver, and orthopnœa, and the term "failure of compensation" is in frequent use. But it has never been realised that these are *end* results, the outcome of years of gradual progressing failure, and that the majority of patients who suffer from heart failure never show these signs.

The principle underlying the methods described for detecting the inefficiency of the heart is so simple that when stated it becomes at once self-evident. The heart's efficiency depends on the integrity of the heart muscle, and its ability to overcome any impediment that may hamper it in its work. The heart's work is facilitated by the other portions of the circulatory system, and by the nervous system, so that when effort is made the different parts of the peripheral vascular system become modified so as to favour an increased flow of blood to the active part.

The explanation of the production of the symptoms which reveal the heart failure.

Heart failure results from impediments being placed in the way of the heart, so that the muscle becomes exhausted in its persistent attempts to do its work efficiently, and this exhaustion is shown by a sensation of distress, the direct outcome of the exhaustion of the heart muscle (e.g., pain), or by a diminished output affecting the functional efficiency of other organs (e.g., breathlessness).

It follows in estimating the significance of any abnormal sign that the question arises, does its cause embarrass the heart in its work? Valve defects, abnormal rhythms, arterial disease, and diseases of other organs, should be judged only from this standpoint when the

question of cardiac efficiency is concerned. Affections of the muscle of the heart itself can be judged by the manner in which the heart can maintain an efficient circulation when the body is at rest and when an effort is made.

One might put the matter in another way—that no heart would ever fail if its muscle were not given more work to do than it can perform without exhaustion. This statement again is so trite and obvious, that it seems a waste of time to state it. Yet, if the principles underlying it are grasped, its due appreciation is of the greatest help in the treatment and management of patients.

The statement that the functional efficiency of the heart, or the extent of heart failure can be recognised by the answers to a few questions, seems so simple that it might be assumed to have been arrived at from a few casual observations, and not to contain a sufficient knowledge of the heart and its activities.

As a matter of fact, it expresses the results of a profound study of the heart's function and activities, such as perhaps has never been carried on in regard to any other organ, and I dwell upon it somewhat insistently, because it demonstrates how medicine can be rendered simpler in its practice, and more efficient in every respect whether of diagnosis, prognosis or treatment, when there is an understanding of the laws that govern the symptoms of disease.

The application of simple principles in practice.

It is some twenty years since this manner of observing the failure of the heart was evolved. After that it was necessary to test it, and at the same time to seek for further evidences in its support or for its modification. When in general practice, I had for patients many of the decent living, hard-working classes, on

whom the test could be very effectively applied, inasmuch as they had to follow their occupations be their hearts good or bad, so that, as years went on, experience fully confirmed the soundness of this view. When in later years, in consulting practice, I saw large numbers of patients with hearts damaged or supposed to be damaged, the application of this principle was of the greatest help.

I have been brought intimately into contact with the views and practices that are prevalent in different sections of the medical world, and I found the utmost confusion prevailing as to the significance of the signs detected in the heart. Great numbers of patients had their lives restricted and had to submit to prolonged treatment because of the detection of some sign. The popular idea is that, if the heart shows an abnormal sign, or what is taken as an abnormal sign, some obscure danger looms in the future, so that patients willingly submit themselves to restriction and treatment. The idea being shared by many doctors, it is easy to see what scope there is for many methods of treatment. It might have been expected that the employment of instruments of precision would have put a stop to all this confusion but the reverse has happened. The introduction of these instruments has so much impressed the public that they are not satisfied unless they are examined by them, and many doctors, who employ these instruments, though they may detect the abnormal signs, have not learnt how to estimate their significance.

If I were to cite a fraction of my experience of the manner in which symptoms are misunderstood it would scarcely be credited, and I only say that the confusion is almost incredible, and the introduction of so-called scientific methods has increased the confusion.

The need for simplification is urgently called for in

the practice of medicine. The remedial measures for the treatment of heart affections are legion in number, and the vast majority are absolutely useless. If the principles I have described be understood, viz., that the danger in every case depends on the possibility of heart failure, and that heart failure only comes about when a heart is forced to do more work than it is fitted for, we are at once placed in a position of pursuing a rational line of treatment, viz., to relieve the heart by easing its burden. As heart failure is usually treated by rest, accompanied by a bewildering variety of adjuncts, the benefit which comes from the rest is attributed to all the various adjuncts. Sometimes, as in auricular fibrillation, the heart failure is due to an excessive rate, and the administration of a drug that slows the heart speedily gives the heart a rest. But the drugs capable of doing this are few, and their action is limited to a few peculiar diseased states. Excessive rapidity in the majority of cases is not amenable to such drugs, and is usually due to the heart being irritated from affections of other organs, as in Graves' disease. The recognition of this leads to an inquiry as to the cause of the increased rate, and the treatment is thus directed to the removal of the cause.

A few illustrations of the application of these principles in practice will demonstrate how medicine can be rendered simple and yet effective in practice.

The recognition of the phenomena produced by heart failure forms the most reliable grounds on which to base a rational treatment and mode of life. As these signs are often provoked in hearts with a limited response to effort the persistent overworking of the heart leads to its more speedy exhaustion. If elderly people who show signs of angina pectoris when making an effort are instructed to stop all exertion on the first sign of distress, the pain may never recur. On the other hand

they may indulge in as much effort as they care, so long as they suffer no distress. In this way the patient recognises his own limitations, and can safely exercise within those limits. This simple method will strike anyone who understands the matter as being reasonable, and based on sound principles, and its practical advantage is of the greatest value, seeing that a great many patients suspected of having angina pectoris have imposed upon them frequently unnecessary restrictions based upon no intelligent principle.

Many children are made to lead a restricted life because the doctor discovers an abnormal sign—a murmur or an irregularity, or the child may have fainted. Children are very sensitive to distress. When the heart is exhausted distress is induced, and children instinctively cease their efforts on the first sign of distress, so that no harm is ever done by the child when left to itself. As there is much confusion as to what signs in children are normal and what are abnormal, and a child cannot describe its sensations, a clear conception of the meaning of exhaustion of the heart enables one in all cases to leave the matter in the child's hands. Even when the heart is damaged and its efficiency impaired, no harm will ever result if the amount of effort is left to the child's own judgment.

Some years ago I was consulted by a man of 68 years of age. He looked fairly well, but had a slightly enlarged heart with the irregular pulse characteristic of auricular fibrillation. When I asked him what amount of exercise he was able to undertake, he answered that he could not tell, as a year ago he had been seen by two distinguished physicians who forbade him to take exercise, and he had been carried up and down stairs to bed ever since. I requested him to go and walk about till he got short of breath, and return to me in a few weeks time. He did this, and stated he

could walk a couple of miles in comfort. I then told him he was to take any kind of effort he liked, provided he slowed down or stopped as soon as he became short of breath. He asked me if he could cycle, as he was very fond of it, and I told him he could cycle if he followed the same instructions. He reported himself from time to time. He went to Australia and back when 70 years of age; had a severe illness from appendicitis, resulting in an operation, at the age of 72; and when last I heard of him he was 75 years of age, and in fair health. During all this time the auricular fibrillation had persisted.

A young doctor consulted me. His story was that he was in good health and engaged in general practice up to a year before. He happened to feel his pulse and recognised that it was irregular. He became concerned as to its significance and journeyed to his old medical school and consulted his teacher. This physician also recognised the irregularity, but not being clear about it, sent him to have a record taken by the electro-cardiograph, which had been recently introduced. The doctor in charge of this instrument took a record and found a peculiar irregularity which he and the physician did not understand, but over which they shook their heads and told the patient that probably it meant nothing, but that he had better be careful. This vague advice rendered the patient very nervous, so he gave up practice and took a hospital appointment in order that he might spare his heart as much as possible. He walked about carefully for short distances, but found he was becoming easily exhausted, and had palpitation, and thinking his heart was getting worse, consulted me. He gave no history of previous illness, and had led an active life, and could take plenty of exercise till a year ago. I felt his pulse and recognised the irregular action. I asked him to breathe

slowly and deeply, which he did, and I found that the heart's rate increased on inspiration and became very slow on expiration—in fact, an excellent example of the youthful type of irregularity—a physiological phenomenon, indicative neither of disease nor of impairment (Figs. 16 and 16A). It was evident that the whole train of symptoms was the outcome of fear produced by the vague prognosis.

Six years ago I was consulted by a man 60 years of age for attacks of a pain which he described vaguely as occurring over the upper part of the abdomen and chest. Left to himself, he gave no clear idea of the situation of the pain or what circumstances caused it. On careful inquiry I elicited certain facts which gave me a clear perception of the nature of his symptoms. By appropriate questions I obtained a good idea of the manner of onset of the pain, the situation in which it first appeared, and the region into which it spread, how long it lasted and the circumstances attending its production. A physical examination revealed no abnormal signs in any organ of the body, beyond those senile changes which we are accustomed to associate with a man of 60 years of age. There was no albumen in the urine.

The symptoms were to my mind characteristic of an early stage of one form of angina pectoris, and I informed the patient that they indicated slight degenerative changes in the arteries supplying the heart muscle; that in the future he would not be able to undertake as much effort as he had been accustomed to, and I gave him directions as to how he should manage his life, and his diet.

He was one of those men vain of their health and strength, and was evidently displeased when I told him that these were signs of advancing years which he could never get rid of, so I told him if he neglected

these warnings the exhaustion of the heart would proceed at a more rapid rate, and the pain be more and more easily provoked, whereas if he followed my instructions and lived at a lower level, he would have many years of useful life before him.

I did not see him again for nearly two years, when he again called upon me. He was much worse, the pain at times being very distressing, and provoked now so easily by effort that he could only walk a short distance. I asked him if he had followed my advice, and he replied that he had not. When I asked why he had not done so, he told me he was disappointed when I told him the nature of his trouble, and on discussing the matter with a friend, this friend told him of a celebrated German physician who would give him a far more thorough examination than I had given. So to Germany he went, and there he was subjected to a very thorough examination—not only by the physician himself, but by experts who gave him test meals, and bismuth meals, with X-rays examinations of his intestines, of his heart and blood-vessels, and other parts of his body. He was examined with blood-pressure instruments and the electro-cardiograph. His blood, urine, and fæces were examined chemically and microscopically, and he was subjected to a number of mechanical devices. Altogether, the visit lasted five days, during most of which time he was under examination. The physician finally saw him, and told him that the examination revealed no disease of the heart, which was perfectly sound, but there was some error in metabolism, and it was to this his sufferings were due. For treatment a special diet was recommended, with the assurance that this would cure him of his complaint.

The patient was greatly impressed with this examination, and expressed unbounded admiration for the scientific methods of the Germans. When I asked him

had he benefited by the treatment, he grudgingly admitted that though he had faithfully followed the instructions, he had not improved, and, in fact, had got worse, and it was because my prophecy had come true that he had returned to consult me.

The admiration which this patient had for this style of examination is so universally shared, even by members of the profession, as being "scientific," that it threatens to become a method established by those in authority. Yet it is the best instance of psuedo-science that we can find.

It would seem that it is not appreciated that I had used a method specially adapted to clinical medicine, namely, the experience of many years' observation in such cases. If the reader will have grasped the steps I had taken to investigate this type of patient, he will recognise what a long training I had to undergo to enable me to estimate the meaning of the essential symptoms in this case—that is, the nature of the signs evoked by an exhausted heart.

Where the method fails.

In citing the experiences which demonstrate the success of the application of the method for estimating the degree of heart failure, I recognise the danger likely to result from shutting one's eyes to the cases where it has failed. To this, however, I have given a great deal of attention, and can recognise the type of case where the method fails. These exceptional cases are those in whom attacks of heart failure or of death suddenly occur. In both instances the cause is usually due to the inception by the heart of an abnormal rhythm. I have witnessed the beginning of attacks of heart failure, and perceived that it was due to the onset of an abnormal rhythm such as auricular fibrillation. I have watched the progress of heart failure till death

ensued. I have seen numbers where the failure did not reach such extremes, but I am totally unable to foretell when such an event as the onset of an abnormal rhythm is likely to occur, though one may suspect the tendency in certain cases. The cause of sudden death is almost certainly due to the onset of an abnormal rhythm, probably ventricular fibrillation—the occurrence and persistence of which is inconsistent with life, as the force of the ventricular contraction fails to maintain the circulation. In rare cases I have noted, heart failure set in unexpectedly with a normal rhythm, as in aortic regurgitation, and has proceeded rapidly to a fatal issue, and I failed to anticipate the occurrence, and yet do not know why it occurred.

In heart failure setting in when a person is in bed, as an elderly person with a broken leg, or in the later stages of an illness such as typhoid fever, the method described cannot, of course, be applied, but the symptoms shown in these cases have been carefully studied, and though of a different kind, are the same fundamentally, being the evidence of an inefficient circulation in remote organs. In like manner, the heart failure in acute diseases has been studied, and I do not refer to them here more fully because, while favouring, they do not advance the argument.

It might seem that such a simple means of detecting heart failure would be readily accepted, particularly as it is the result of a prolonged inquiry involving a study of the physiology of the heart and of many diseased states. There are, however, only a very few who have accepted the teaching and trained themselves to apply it. Its very simplicity baffles people, particularly those who have been accustomed to put their faith in mechanical devices. It is manifest that students who are compelled to learn the methods described on pages 35 to 39, or even a fraction of them, cannot

realise that all this part of their education could be rendered unnecessary. Moreover, the public have a great belief in obscure and tortuous methods. This is really a part of human nature, revealed in the untutored savage who, in his troubles, makes for himself a graven image to worship, and in the faith that to-day finds solace in elaborate rites and ceremonies. The public demand from the mysteries of medicine what they require in the mysteries of religion, and where there is a demand there will always be those to meet it. So we need not be surprised if the public look with trust on the magnificent temples dedicated to research and ignore the simple methods of the practitioner with whom lies the future of medicine.

CHAPTER VI

THE USE AND ABUSE OF LABORATORY METHODS

The dominant position of laboratory methods in clinical medicine.

In endeavouring to give a clear idea of what I consider to be the methods by which medicine may become a science, I may seem to attach too little importance to laboratory methods. This is not my intention, especially as I have myself used some of these methods, indeed, more than most physicians; and I have repeatedly resorted to laboratory workers for assistance in solving clinical problems. What I wish to do is, to show that there is expected of these methods what they are incapable of giving, and the introduction of their systematised and universal employment, unless carefully regulated, hampers the progress of medicine. The limitations and defects of these methods have not been realised, and while their use is supposed to be evidence of progress, it is often but a procedure demonstrably opposed to the best interests of medicine. To the superficial observer, the introduction into the practice of medicine of instruments for accurate measurements seems very reasonable, and what could be, apparently, more scientific than bodies of specialists grouping themselves together for the more thorough examination of their patients, or physicians and surgeons sending their patients for a report from an ever-increasing number of men who employ special methods of examination?

The idea that these procedures indicate progress has become so widespread that it is necessary to inquire whether it is for the good of the patient, and for the best interests in medicine.

The kind of information revealed by laboratory methods.

The first point to realise is the nature of the information which these laboratory methods give. It will be found that the symptoms which they reveal belong to the first group in the classification given on page 137, i.e., they belong to the group of structural symptoms. I have already dealt fully with the limited amount of information to be derived from this group, and it is because the significance of this class of symptom has not been recognised that the results of the laboratory methods of examination are often rendered of little value, when not actually misleading.

Effect of the introduction of the stethoscope into practice.

An excellent illustration is afforded by the first mechanical aid to clinical diagnosis that has been widely adopted. Its history and the uses to which it has been put reveal the attitude of the profession towards the introduction of mechanical devices. About one hundred years ago auscultation began to be used to detect the sounds of the heart, and it revealed to the earlier observers that in healthy individuals the sounds were clear and well struck, while in people suffering from manifest heart failure, the sounds were not clear, but were muffled and modified in a manner which was described as "bruits." At once the conclusion was formed that bruits were of a serious significance. This conception arose at a time when the cause of these bruits or murmurs was unknown. The outcome of this hasty generalisation imposed upon the profession a perverted and erroneous conception of the significance of

murmurs, which has permeated every part of the world where auscultation is employed, and to-day misleads the profession in all countries. Not only that, but, after the association of murmurs with certain valve defects had been made out, it introduced the back-pressure theory of heart failure, a theory which has diverted attention from the essential nature of heart-failure. I have shown how heart failure is essentially a matter of the efficiency of the heart muscle, a view which the physicians one hundred years ago had recognised. I think it can be said without exaggeration that the misuse of the discovery of auscultation has not only directed the prosecution of inquiry into wrong channels, but it has caused harm to a great many people, and is doing so to-day, in that doctors have never been taught how to estimate the significance of the signs obtained from auscultation.

The reason for the failure of laboratory methods.

This lack of judgment evinced in the discovery of the stethoscope will be found in relation to every mechanical device. Fifty years ago the sphygmograph came into use, and was hailed as another instrument for the better understanding of the heart's work. The character of the tracings of the radial pulse was found to vary, and speculation as to their causes became rife. The percussion wave, the dicrotic wave and dicrotic notch, show many variations, and conclusions were drawn from these variations, just as to-day we find the variations in the height of the waves in an electro-cardiogram being used as a basis for estimating the heart's efficiency, while we are still ignorant of their cause, and when, as of old, the method by which their value should be assessed has never been understood. The same lack of knowledge of how to find the significance of symptoms, causes the clinical use of laboratory

methods to be applied for purposes they are incapable of fulfilling. The disappointing results of the employment of bacteriology in clinical medicine is becoming every day more and more apparent. The discovery of the tubercle bacillus was hailed as a discovery that was to have a profound influence on the prevention and cure of consumption. As an essential agent in producing consumption its discovery has added greatly to our knowledge, but the application of this knowledge to the prevention and cure of consumption has failed, and for the reason that the kind of knowledge necessary for its proper employment in clinical medicine has not been obtained. Attempts are continually being made to apply this discovery to useful purposes, but it requires no prophet to foretell that these attempts will continue to fail until all the factors in the production of the disease are recognised, and these will never be discovered by laboratory methods alone.

Since the introduction of the stethoscope, a number of auxiliary methods, most of them the outcome of laboratory research, will be found in the list of methods on pages 35 to 39. While it may be claimed that we may have one hundred new methods for investigating disease in the living, each of which adds to the sum of our knowledge, it must also be recognised that we have one hundred more ways for going astray. It can further be said that, while the benefit to the patient is often doubtful, the employment of these methods, after the manner of to-day, is frequently harmful.

Loss of contact with the patient deprives the physician of a source of much useful knowledge.

In the employment of laboratory methods, so much attention is given to the results of the instrument that the patient is often ignored. This is no rash charge, but a recognition of a great defect in this kind of in-

vestigation. When one gets a report from a specialist who devotes his time to a particular field, the patient is not mentioned except incidentally, just as a report of a urine examination made in a clinical laboratory. The reports of test meals and X-rays examinations, of the examination of the blood and of the heart, and so forth, which have been made by specialists in their departments, usually contain no reference to the phenomena perceptible to the unaided senses. When a patient consults a specialist in the belief that for his particular complaint the particular specialist is the doctor for him, the specialist can only see but a small part of the patient's state in many instances, for disease is rarely limited to one organ.

Sometimes a physician, under the influence of this laboratory conception, sends his patient to a series of specialists and receives their reports, and on the strength of these reports, with his own examination, believes himself to be able to deal effectively with the patient's complaint. Apart from the fact that it is manifestly impossible for a physician to assess the value of a sign which another has to detect, there is a very important source of knowledge which is lost in this handing over the patient for examination by a series of experts.

In the investigation of the movements of the heart, blood-vessels and the liver, which I carried out with the clinical polygraph, I became so familiar with the characters of the different movements that I could recognise them by the unaided senses. This knowledge was acquired by comparing what I saw and felt with the graphic records. Physicians who now-a-days send their patients to be reported on by experts in the use of the X-rays and electro-cardiograph, can never acquire this kind of knowledge, a knowledge which it is manifest is absolutely essential to a teacher who would

instruct his students to become intelligent practitioners.

The handling of the patient and the perception of phenomena by the doctor's unaided senses, reveals an extraordinary fruitful kind of knowledge. To one who takes a tracing of the pulse, the familiarity with the different kinds of artery and the different characters of the pulse, as perceived by the finger, yields a peculiar kind of knowledge. If these impressions are compared with the tracings, then a relationship can be established between what is felt by the finger and what is presented by a record. This familiarising of the doctor with the impressions given by the finger leads to the acquisition of a knowledge of real value. It is also shown in percussion. The experienced physician gains information, not only by the sense of hearing, but by the curious sense of resistance felt by the percussed finger over regions of varying density. The *tactus eruditus* is no fanciful idea, but a factor of importance in the perfecting of that necessary instrument to clinical medicine—the trained physician. What is true of the pulse is true of every sign which is capable of recognition by the unaided senses. The eye has to be trained to see and the finger to feel, and this can only be brought about by long and patient education. It is due to the systematic neglect of the employment of the unaided senses that that great field of clinical information, revealed by the viscero-sensory and viscero-motor reflexes, has been overlooked. This neglect will not only continue, but the powers of the unaided senses as clinical agents will diminish by the substitution of laboratory methods. Of late years an idea has arisen that a laboratory training is of great value for one who intends to become a physician. In my experience I have found that it unfits a man for his work as a physician for the reason that, not only does the laboratory-trained man fail to educate his senses, but he puts so much trust in his

mechanical methods that he never recognises their limitations, and he fails to see that there are other methods, which are essential to the interpretation of the signs of disease, of which he has absolutely no experience.

The training of the senses.

A thing that strikes anyone, who gives attention to the matter, is the curious knowledge which some physicians and general practitioners acquire after many years' practice. It enables them in an unconscious manner to estimate the patient's state with remarkable precision. The knowledge is undefinable, and they are unable to express the reasons in language sufficiently clear for the uninitiated to understand. The real source of this knowledge is the familiarity, derived from experience, of the appearance of the patient when stricken with an insidious disease, a subtle alteration in the expression of the face, or a slight wasting, or a faint contraction of some of the muscles of expression, a faint change in colour, coupled, it may be, with an alteration in the patient's temper, ideas or voice. Knowing the patient before these changes occurred, the attention is arrested by the alteration. One or two gross illustrations may help to understand this indefinable aid to clinical medicine.

When I was a resident in hospital a powerful young man was brought in evidently ill. A few hours before he had a severe rigor and he now had a temperature of 103 F. An examination revealed no abnormal sign. While we were puzzling as to the nature of the trouble, our teacher came into the hospital and I brought him to this patient. He merely looked at his face, felt his pulse and turned away and said, "Going to have pneumonia," and then, in a low voice, "He will do no good." The diagnosis was correct. The patient

developed a pneumonia and died in a few days. For long after I puzzled over the reason for this experienced physician being able to come to such a correct opinion. It was many years after, when I had seen a number of similar cases, that the peculiar features of this type of pneumonia became apparent to me. The look of distress and the working of the alae nasi are typical of many pneumonias of a favourable type, but in the dangerous cases there is a slight dusky tinge of the countenance, with a faint blueness of the lips, and the pulse is large, with no sustained force, and rapid—a pulse difficult to describe, but the feeling is peculiar, and corresponds to a high wave with little or no dicrotic wave in the sphygmogram. Such is the somewhat coarse analysis of undefinable impressions.

These undefinable impressions are of real service. Thus I was once called to see a patient in consultation. When I entered the house I found the doctor there very sad and depressed, and he muttered to me "too late." I asked what was the matter, and he said that he had attended the patient in her confinement four days ago, and she had developed puerperal fever, with a temperature of 103 deg. F. and was dying; that it was the first case in his practice, and he felt he was to blame. On entering the bedroom, the patient's face struck me as being bright but very anxious; there was a red flush on her cheeks. When I felt her pulse I looked at her, and she was gazing at me in a frightened way, and I smiled at her. She looked surprised, and said, "Doctor, am I not dying?" "No," I replied; "You'll live to have a dozen babies yet." She made a good recovery.

A short time after I was called to see another patient. I met the doctor, a clever young man, who had been five years in practice. He told me there was nothing to fuss about. The patient was confined a week ago, and there was a slight rise in temperature to 101 deg.

F., but the patient's friends were anxious and troublesome, and he called me in to reassure them. When I entered the room I saw that the patient's face was very different from that of the last-mentioned patient. She was dull and apathetic. There was the faintest sallow tinge in a somewhat gray countenance, and the pulse was small. There was nothing else revealed on examination, and the doctor was incredulous when I told him that it was a case of puerperal fever, and that she would be dead in 24 hours—which turned out to be correct. No blood count, no bacteriological examination of the discharge or the blood, no instrumental method of examination of the heart, could tell me as much as a glance at the face and the feel of the pulse revealed.

How was this knowledge acquired? At the time I entered practice Semmelweiss' great discovery had not been utilised in this country, and it was after I had been a few years in practice before I was acquainted with it. During these years I had ample opportunity of observing puerperal infection. After I adopted the plan of thorough disinfection of my hands, a great change came in my midwifery practice. The practice I had joined was an old-established one, and we dispensed our own drugs, and for that purpose kept a dispenser who had been there since 1870. Some time after I had adopted strict precautions in my midwifery practice, he said to me: "What a curious change has come over your confinement cases. In bygone years we were never without serious cases, usually two or three with white legs, and every now and again a death. My first wife died in her first confinement. Now you never have any white legs, and no deaths."

Although I had but one death from puerperal fever in my own practice, I saw plenty of it in other practices. I was called to a distance once to see a woman

who had been three days in labour. She was elderly, and a primipara. The doctor in attendance had tried to deliver with forceps and failed, and had called in two neighbours who also failed. I managed to deliver her, and left her looking very ill. Next day I was sent for to see her as she was dying, but I could not go, and a colleague went in my place. She was sinking, and unfortunately, he examined her. On his way home, he was intercepted by a man who took him to attend to his wife, who was in labour with her first child. Immediately after the delivery the doctor was called to another primipara in labour, whom he delivered. A few hours later he was called to attend a third woman in labour. Within a week these three women were dead. We studied them carefully and examined one of them after death, but could find nothing beyond these signs of intense depression which ended in death. This is a type of the bitter experience by which the general practitioner is taught his profession.

Laboratory and Clinical Methods compared.

The advances that have been made in special branches, particularly those pursued in laboratories, and by the use of mechanical devices, are all reflected in the field of clinical medicine. In an attempt to be up-to-date physicians use the methods of these auxiliary branches, so that clinical medicine toils laboriously in their rear, and to a great extent becomes subservient to them. It should be realised, and should be strongly and persistently insisted upon, that all these special methods fall far short of the ideal of what is wanted in clinical medicine, and that the pursuit of clinical medicine involves problems, peculiar to itself, which none of these methods can ever hope to solve. There is not a single mechanical or laboratory method ever introduced but has had an extremely

limited sphere of usefulness. Time and again great expectations have been raised on the announcement of some wonderful discovery that was going to have a revolutionary effect upon clinical medicine, but in every case, as time went on, when its sphere of usefulness came to be recognised, it was found to be a very limited one. The impression that the results obtained by a mechanical method are more reliable and more scientific than those obtained by the use of the unaided senses, is but a belief based on a false analogy. It is assumed that because the experimentalist obtains certain records which are capable of demonstration, that these are of more value than the information derived by the doctor from questioning his patient, or by the use of his own senses trained by long experience. Those who reason thus unconsciously compare unequal things. In laboratory experiments, it is impossible to obtain any information from the great field of subjective impressions. In man this is the most important field, and far outruns the field for mechanical exploitation in giving us information as to the nature of disease. The laboratory worker obtains his results by a delicate mechanical contrivance. The physician has to train his senses, and this can only be done by a long process of education, only capable of being acquired by the constant contact with the patient.

The trust in Laboratory Methods leads to the neglect of the patient's sensations.

This faith and trust in laboratory methods renders the physician incapable of appreciating the value of methods peculiar to clinical medicine. I have shown the steps I took to find out a method to estimate the heart's efficiency. To obtain this knowledge methods peculiar to clinical medicine were the only ones capable of solving this problem, for laboratory experiments

have failed to give any exact information. A little consideration will show that this could not be otherwise, as the progress of disease, with its slow development of symptoms, can not be reproduced on animals. After I had worked out the method by which this very important matter should be carried out, I was able to formulate the laws for its application, which were so simple that any intelligent individual could apply them.

But unfortunately, it requires a great deal of experience to elicit from patients an intelligent description of their sensations, and an experience which will enable the doctor to recognise those different sensations; and this is just one of those essentials required in the examination of patients which the laboratory-trained physician seems unable to acquire. As a consequence of his inability to acquire this knowledge, he ignores the information which it reveals, which I have shown is one of the causes of the neglect of the study of the early signs of disease. Speaking from a very extensive knowledge of disease of the heart, I am every day more and more convinced that the method described is the only one by which the efficiency and degree of inefficiency of the heart can be revealed in a great many cases. Anyone familiar with the physiology of the heart will recognise that it is based upon fundamental principles, and those who have adopted it in an intelligent manner support me as to its value.

The reason I dwell upon this method of estimating the heart's efficiency is that I find many physicians and a few physiologists unable to understand it. When dealing with the subject in their writings, it is not only ignored, but, from the methods they recommend, it is revealed that they have not grasped the elementary principles necessary to the understanding of this subject. I do not say this because of any personal

feeling of a neglect of a method devised by myself, but in order to bring out, prominently, a glaring defect in clinical examination by laboratory methods.

The employment of blood-pressure instruments affords an excellent illustration in showing how completely their place in clinical medicine has been misunderstood. The figures obtained, which are referred to as representing the systolic or diastolic pressure, do not actually represent these pressures, but have only a more or less close relation to them. It is true that the figure does bear generally such a relation that a reasonable idea may be obtained of the difference between different individuals, and can be used for indicating variations that occur, as an effect of disease or of treatment. Certain features that the trained finger recognised, and which were wont to be described by such terms as a hard pulse or a soft pulse, are now given in definite numbers of milligrams of mercury, and it is no doubt helpful in many instances to express the matter in this way.

What the factors are, however, that cause variations in pressure has not been discovered. The heart supplies the force but the variations of pressure are due to the play of the vaso-motor system of nerves on the vessels, so that no information of the state of the heart can be obtained by blood-pressure measurements. Doubtless some peculiar variations may be detected which are found associated with certain cardiac conditions, but it should not be taken as a guide to the heart condition, for other evidences will invariably reveal, with more directness, the heart's strength.

If one recognises that the nature of the phenomena, revealed by the blood-pressure instrument, belongs to the group of symptoms which I have classed as structural, the limited nature of the information which they give will be understood. This limitation has not been

appreciated, and the same extravagant and misleading claims are made for this method as were made for the use of the stethoscope and sphygmograph. Because people, seriously ill, showed bruits, therefore bruits were dangerous, so the reasoning is now that because people seriously ill show a high blood pressure, therefore a raised blood pressure is dangerous. Just as in the case of murmurs, so here the methods essential to assessing the value of the signs were never taken—i.e., individuals, with different degrees of blood pressure, have never been watched for a sufficient length of time to see what happens, nor have they been observed with that system, which I showed was necessary to understand the meaning of every sign, by the careful observation of associated phenomena.

With this imperfect knowledge of the sign, the employers of this method have submitted many patients to modes of treatment, which, in the main, had as little justification as the old method of giving digitalis because a murmur was detected in the heart. Drugs that have a reputation for lowering or raising blood pressure are freely administered, without any knowledge as to whether the drug possesses the qualities attributed to it. Systems of diet and modes of life have been imposed upon patients, frightening them and restricting them, because a sign was detected, whose significance was never understood.

The place of laboratory methods in clinical medicine.

I have dealt somewhat fully with the danger that may result from the employment of laboratory methods in clinical medicine, and it may be assumed that I disapprove of them, but I am far from doing that; what I wish to guard against is their injudicious use.

It was some time before I realised the limitations of mechanical methods, such as the graphic records of the

movements of the heart and vessels. I began drawing conclusions from the features of these records, and it was only after some years' experience had demonstrated the futility of my judgment, that I began to get a glimmering of where the fault lay. Carrying on my observations, there gradually dawned upon me that distinction of the phenomena which forms the basis of the classification of symptoms which I have given, and which incidentally puts this kind of information, yielded by instrumental methods, in its rightful place. In the picture of disease these phenomena get into their proper perspective.

It must become evident, with the development of mechanical aids and other methods, that there is no end to the methods that may be employed in investigation. This is seen from the large number of methods recommended for the student's instruction on pages 35 to 39, and this only gives a fraction of the methods that are available, to which additions are constantly being made. This indiscriminate way of using these methods not only confuses the purpose of medicine, but at times reduces its practice to a farce, so that it is time a halt was called, and the real value of these methods estimated.

For the discovery of new facts connected with the phenomena of disease, there will ever be an improvement of, and addition to, the methods—and this should be encouraged. But before these methods can be employed in clinical medicine, and recommended for use in practice, certain preliminary steps must be taken.

Before a new method is introduced into medicine its limitations must be realised and the nature of the knowledge it reveals must be demonstrated.

The first step is to find out the value of the facts which the new method brings to light. No instrument

should be allowed to be used *in practice* until the inventor demonstrates the bearing the facts which it brings to light has upon the diseased condition it reveals, that is to say, he must be prepared to show the effects of the cause of this new sign upon the health of the patient, and show that its recognition gives a guide in treatment. This evidence must not rely on theoretical considerations, but must be based on a report of a sufficient number of individual cases, which have been followed long enough to verify the claims, and in whom the associated phenomena have been studied in accordance with the methods peculiar to clinical medicine.

It will be asked, no doubt, "are we to wait while these observations are being carried out, and thus lose the valuable aid which the discoverers claim for it?" If anyone will read the true story of every mechanical device, he will discover that the predictions made at first as to their value have never been fulfilled. I have already referred to the harm which the neglect of this advice did to medical science in the case of the stethoscope, and which is being done to-day in the use of blood-pressure instruments, the X-rays and the electro-cardiograph. Everyone knows what harm has been done by the misinterpretation of the shadows in the lung and other organs revealed by the X-rays, and by the conclusions drawn from a reaction to tuberculin. The whole history of the tubercle bacillus is but an exemplification of the neglect of this precaution, and the mischief which the random use of mechanical methods has done is incalculable.

After a new method has been discovered steps must be taken to supersede it.

The next thing the discoverer of a mechanical device must do after he has recognised its use in clinical medicine is, *to get rid of it in practice.* This seems an

absurd recommendation, but it is vital to the progress of clinical medicine. If, for instance, all those methods and devices described in pages 35 to 39 were necessary to the practice of medicine, how would it be possible for any doctor to do his work?

The way this can be achieved is by employing the law of associated phenomena described on page 129. I have already pointed out that impaired health is always associated with a number of symptoms, and it is doubtful if there is any sign of value revealed by a mechanical device which has not associated with it other signs produced by its cause, or produced in some other way which will reveal its presence, or from which its presence can be safely deduced. This idea occurred to me when in general practice. It was soon evident to me that if the information I had collected in regard to irregular action of the heart was ever to be of use to the general practitioner, I must find out means of recognising the different forms by the unaided senses. To this end I studied each case, noting at the time of the irregular action the character of the pulse, the movements of the veins in the neck, and the sounds of the heart. By these means I was gradually able to recognise the majority of irregularities without artificial assistance, and have shown that these signs can be readily taught to students, if the teacher himself has grasped the significance of this method of inquiry, and he can modify his teaching to suit the circumstances.

It must not be imagined that such a procedure is suited only to cardiac affections; it can be applied probably to every mechanical device. I have already shown that bacteriology will be restricted in its sphere of usefulness unless the clinical symptoms, provoked by the action of an organism, are recognised. The essential matters connected with diseases of the stomach would be comprehended if the sensations of the

patients were more carefully studied and co-related with the findings resulting from X-ray and test meal examinations, particularly if we get to know more of the phenomena which I have called the viscero-sensory and viscero-motor reflexes. Indeed, such an attempt will advance our knowledge greatly, for there is little doubt that the affections of the stomach are often secondary to abnormal conditions in other parts of the intestinal tract.

This co-relation of the clinical signs with the results of the clinical and microscopic examination of the urine will be found absolutely necessary to the detection of kidney diseases. If one looks at the innumerable methods that are used for the detection of defects in the urinary secretions, it is manifest they can never be fruitfully employed until the clinical signs are studied along with them, and substituted for them.

The purpose of laboratory methods.

It will be seen that laboratory methods can only reveal a fraction of the symptoms in any given case, and it is necessary in every case to search for signs perceptible to the unaided but trained senses. If there be no clinical sign, then it may be taken that the fact, revealed by the method, is of no importance so far as the health of the patient is concerned. This is a general rule; here and there exceptions may be found. For the prosecution of limited fields of research, laboratory methods will always be necessary. It is manifest, for instance, that the accurate estimation of the effects of remedies on the heart can only be acquired by some instrument that records the movements of the heart and blood-vessels. These and other methods are necessary for research and for purposes of demonstration. For teaching they should be used sparingly, and chiefly with the view of training the senses, and not as a substitute for the senses.

Matters necessary to be understood in the employment of laboratory methods.

There are three considerations which should be borne in mind by those who employ laboratory methods in clinical diagnosis before the new method should be recommended for use in the practice of medicine.

First.—The sign revealed by a laboratory method must be studied so that the mechanism of its production is understood.

Second.—The value of the sign must be assessed so far as it bears upon the patient's future health.

Third.—The method, if instrumental, must be supplanted by ascertaining the associated phenomena perceptible to the unaided but trained senses.

CHAPTER VII

THE EARLY STAGES OF DISEASE

ONCE the importance of a full understanding of symptoms be grasped, methods by which they can be used for the better detection of disease will gradually be evolved. In seeking to achieve the aim of medicine, namely, the prevention and cure of disease, the simpler steps have been undertaken—that is, the recognition of changes after death, and of the physical signs of disease revealed either by the unaided senses or by some laboratory device. The next step is to recognise the early signs—a step which is necessary before we can advance to the consideration of the conditions that predispose to disease.

The Onset of Disease.

The first appearance of disease in the human body is invariably insidious, with little disturbance of the economy, and no visible signs of its presence. By and by the patient becomes conscious that all is not well with him; there is a loss of that feeling of well-being which accompanies the healthy state. Disagreeable sensations arise, at first vague, but later becoming more definite, and these may become so urgent that he seeks advice. Still no evident sign of disease may be perceived on the most careful examination. By and by the disease, being situated in some organ or tissue, changes the constitution of the part, so that its presence is now

recognised by a physical sign, when the clinical methods usually employed reveal its character.

The sensations of the Patient.

There is at first sight a great similarity in the sensations experienced by patients who feel "out of sorts," and most people see no likelihood of any further knowledge of the complaint to be obtained from a careful inquiry. To the untrained eye the members of a flock of sheep are so like one another that it seems impossible to recognise separate individuals, yet the intelligent shepherd knows the peculiarities of each individual, though he might not be able to give a comprehensive description of the features by which he differentiates them. There are shades and varieties in the sensations produced by disease, which are hidden from the untrained observer. If these were clearly differentiated, and if the mechanism by which they were produced were understood, I feel confident we would get a knowledge of disease long before it has done much damage to the constitution, or has put it in peril. I have shown how some of the more obvious sensations, as pain and exhaustion, have yielded a limited amount of information. There are many sensations which have some very definite cause producing them. A systematic effort made to detect these sensations, to differentiate them clearly, and to note their coming and going, and their association with other phenomena, would throw an unexpected light on the early phases of disease. Of course, in our present state of knowledge the matter seems hopeless, but that is simply because we do not yet understand how to set about the work.

The training of the clinical observer.

The first step is to train the observer. Laboratory workers state that it is necessary for a man to spend

years to acquire the technique for laboratory investigation, and large numbers willingly undergo the training. It requires a great deal more time and application to acquire the methods necessary for clinical investigation, for the observer has to be trained in a school of experience, where qualities of a peculiar kind are necessary. One essential matter is that he must have seen the patient and elicited the symptoms from the patient. He must have watched the patient and noted the progress of disease and the manifestations that accompany its progress, and have co-related the signs during life with those found at operation or death. It will be seen that clinical medicine demands a man of great experience, versed in methods that are peculiar to clinical medicine, and he cannot be trained in a few years, nor can he acquire his knowledge but by the study of the living patient.

That is the sort of training that it is necessary to have before we can hope to understand the early stages of disease.

How to elicit the signs of the early stage of disease.

Seeing that the symptoms produced by disease in its early stages are subjective, the patient must give a coherent account of these sensations. This information can never be obtained without intelligent questioning of the patient by his doctor. This was one of the things about which one of our professors of clinical medicine (Professor Sanders) was extremely insistent. At that time none of us really grasped the importance of his instruction for, then as now, we were all hunting for something we could see, feel or hear. In later years his instructions come back to me with ever-increasing force. He used to tell us to have a clear idea in our minds of the kind of information we wanted, and never to let the patients alone until we got as full an answer

to each specific question as the patient was capable of giving. This implies that the physician has a good knowledge of disease and its manifestations—a thing not possible for the young and inexperienced physician. I recall one particular demonstration of Sanders' methods. When I was his resident physician, a man was brought into the ward very ill with some obscure chest complaint which I could not understand. When the professor paid his visit one of the assistant physicians to the Infirmary accompanied him, as he wished to tell the professor about the patient, who had been under his care three months before. The history was that the man had an illness, characterised by a profuse purulent expectoration, during which he had coughed up a piece of membrane, which the assistant physician said was a cast of the bronchial tube. The professor patiently listened, and then turned to the patient and asked him a series of questions, occupying about five minutes. It seemed to us a tedious and barren procedure. Then, without examining the patient, he startled us by stating the man had suffered for years from a hydatid cyst in the liver. This had suppurated, and the pus had burrowed its way through the diaphragm into the lungs, and had been coughed up. The supposed cast of the bronchial tube was really the sac of the cyst, and if it were examined under the microscope, no doubt we could detect the characteristic hooklets. All this was verified later by a microscopic examination and at the post-mortem examination.

It will be gathered that such a diagnosis could only have been made by a man of very wide experience. As he was also professor of pathology, he had manifestly not limited his studies to the disease on the post-mortem table.

My own training and opportunities have not enabled me to carry the line of investigation very far, as I un-

derstood its full significance too late in life for me to become adept in its use. I had organised at the London Hospital a department connected with the out-patients, in which we were beginning to make some progress with the matter when the war broke out and interrupted our work. At the London Hospital there is an enormous number of out-patients. In the clearing room there is a young physician, who gives a rapid examination of the patients so as to separate them, and send each one to a particular department, surgical, medical, skin, eye, etc. As I sat beside him one day watching the procedure, there came a youth complaining of not feeling well. The busy physician made a hasty examination, and finding no physical sign, prescribed some medicine, and told him to come back in a week. I asked the physician if I might have this particular patient, and he consented. I had recognised that the lad was evidently ill, and so I took him into one of my beds and examined him carefully. I could find no physical sign of disease, nor was anything revealed by the various methods we employed (X-ray, tuberculin tests, blood examination, etc.). We noted his sensations carefully, and kept in touch with him. Two months later he showed a rise in temperature, and speedily developed all the signs of miliary tuberculosis and died. We had intended keeping notes of the sensations of this kind of patient, and after watching a large number, make an analysis of our notes, and see if we could get a relation between the sensations and the oncoming disease.

It is only by some such method that the early signs of disease can ever be recognised, and it is time an organised effort was made to understand this phase of disease.

Illustration of the method for eliciting a patient's sensations.

It is difficult to convey in writing the manner in which the patient's sensations can be elicited, so as to obtain a knowledge of the nature of his trouble. In the description of the manner in which the efficiency of the heart can be ascertained, I have attempted to show how that can be done. I do not pretend to have got very far in other complaints, but in a few instances I have achieved a limited success, chiefly in affections of the heart and in painful affections of the abdominal organs. The following instance illustrates how the subject should be pursued.

I saw a man in consultation with a physician, a surgeon, and a general practitioner. The story was that the man, aged 40 years, had suffered for four years from violent attacks of pain in the abdomen lasting for one or two days, every four or five weeks. He had consulted several physicians and surgeons, and ultimately a diagnosis of gall-stone colic was made, and he was operated upon. No gall-stones were found and no relief was obtained from the operation. When I saw him he was free from any suffering and there was no physical sign to be detected. He had been examined by the X-rays. I proceeded to question him as to the site of pain, and found his statement at first so vague that I could make nothing of it. But I got him to think carefully of the place where the first sensation of discomfort appeared, and to show me the lowest part of the belly in which the pain was felt. After a good deal of questioning, I was able to state that the patient suffered from some obstruction at the lower end of the small intestine, and that it was not complete, and would probably be of the nature of an adhesion associated with an old appendicitis. An operation was performed, and this diagnosis was confirmed, the appendix was removed and the bands broken down. The

patient made a good recovery and the pain did not return.

The reason I had for making this diagnosis was that I found that the pain first started slightly at the lower part of the epigastrium, and descended, with increasing severity, to the lowest part of the umbilical region, beyond which it did not go. The character of the pain, coming on in wave-like attacks, indicated that it was due to the contraction of some hollow viscus, that as it persisted most severely in the central region of the abdomen, gall-stones and renal colic could be excluded. The only hollow viscus that could cause the pain in this region was the small intestine, and the radiation of the pain indicated that the peristaltic wave had started high up and stopped at the end of the small intestine. (*See* Fig. 4, page 72). As a peristaltic wave always stops at the point of obstruction, and never descends below it, one could infer with certainty the part of the bowel where the obstruction takes place.

It will be gathered from this simple illustration that the examiner had to have a very clear idea in his mind of the kind of information that was required. The knowledge necessary for this purpose had been attained after many years' observation, and the questions had to be framed in such a manner that the patient could understand them and describe the facts necessary to an accurate diagnosis.

CHAPTER VIII

SECONDARY DISEASES

Arterial Degeneration.

My knowledge is so limited that I am unable to give a classification of diseases that would be any advance on those in common use. But I dimly perceive that an investigation, which includes the lines I have employed, will lead to an understanding of the principles underlying the production of symptoms, and that a classification of disease, of much simplicity and of great practical service, will be obtained. I can see, for instance, that many ailments which figure as distinct diseases of one organ are really produced by an affection of another organ. Many of the diseases that figure in surgical textbooks are undoubtedly end results or secondary diseases. A good illustration is found in the effects of a diminished supply of blood from arterial degeneration. The degeneration varies in severity in different regions, so that in one person the arteries of the kidney, in others the heart or the brain suffer most. The usual way is to describe, as separate diseases, the phenomena evoked by each organ. And even the different parts of one organ being separately affected, the peculiar manifestations are described as being due to different diseases. Thus in the heart, arterial degeneration may chiefly affect the ventricle, and give rise to angina pectoris, or, affecting the valves, give rise to valvular disease, or, affecting the auriculo-ventricular

bundle, give rise to different forms of heart-block, and the Adam-Stokes syndrome, or, affecting the auricles, give rise to auricular fibrillation.

This leads to the conception that when we understand better the consequences of arterial degeneration, a great many complaints, at present described as distinct diseases, will be recognised mainly as phenomena, secondary to a deficient arterial supply.

But this is only one step—the inquiry will continue, "Is not arterial degeneration itself a secondary condition?" When we face this question frankly and honestly, we must admit a deplorable ignorance of a very common subject. Probably in late years no matter in medicine has been so much written about and discussed as arterial degeneration and the phenomena associated with it, particularly since the introduction of instruments that are supposed to tell us the blood pressure. There have been books written upon the subject, but they all deal with the disease, so advanced, as to be beyond the hope of cure, and long after any clue can be detected to account for the onset of arterial degeneration. In no instance has there been given an intelligent history of one individual, showing the condition of his health before the occurrence of the arterial trouble, with the phenomena produced at its onset, or which accompanied its progress. There are, however, innumerable suggestions as to the cause, which are mainly guesses, and lines of treatment are laid down for the early stages by writers who evidently have never seen a patient in the early stage.

Here then is an obvious defect in medical knowledge, and one which can only be made good by working on some such lines as those I have indicated.

Diseases secondary to affections of the Intestinal Tract.

I have made but little progress in my investigation

of the early and predisposing stages of arterial disease, chiefly because, by the time I had learnt how the inquiry should be pursued, I had lost the opportunity, on giving up general practice, of following the cases I had been watching. I had, however, pursued this subject in another way.

In the close scrutiny of patients, and in watching them for years, it is surprising to find that their early symptoms are ultimately found to be due to diseases which one had never suspected had any relation to their original complaints. Cases of malignant disease present, for many months, symptoms so indefinite that they are often classed as neurasthenia, even by experienced physicians, and patients may have their complaints labelled for years with such a term until some striking event, such as coughing or vomiting blood, or a perforated gastric ulcer reveals the true nature of their trouble. On the other hand, many diseases are superadded or secondary to complaints, which pave the way for their introduction. For many years I had such occurrences in my own practice, but for a long time my knowledge of how to investigate the symptoms of patients was so deficient that their study but added to the confusion of my ideas. As time wore on, and I gradually obtained more and more insight into the nature and significance of symptoms, some light began to be shed on certain of the phenomena, and I was gradually led to distinguish one class of case, which presented at first sight a bewildering variety of phenomena. On closer investigation I found that all had a common complaint in some defect of the digestive tract.

The type of individual was usually pale and poorly nourished; some with signs of digestive trouble of various kinds; others with no consciousness of digestive discomfort, though they have "had to be careful of

what they ate"—always a suspicious circumstance. But these were not usually the symptoms of which the majority complained when they consulted me, and were only elicited during the course of examination. The symptoms which caused them to seek advice were usually complaints of exhaustion, being easily tired with bodily or mental effort. Some had more specific signs, such as palpitation, aching in the chest and arms, and even attacks of pain, often described by doctors who had seen them as angina pectoris. In others, mental symptoms, such as irritability of temper and depression, were present, some being looked upon by psychiatrics as borderland cases of melancholia. I had sorted this class of patient out from among others, and being uncertain of their nature, had classified them, provisionally, under the term "X-disease," to call attention to a distinct class, and at the same time to indicate that the true nature of the condition was unknown. When I was a consulting physician in London, I saw a very large number of this type, and many of them had consulted numbers of specialists, and had been treated by each for a different complaint.

In my further studies of these cases, watching their development, I came to the conclusion that there was some affection of their digestive tract which interfered with their nutrition. This was shown by their spare bodies. The mental state and the cardio-vascular condition appeared to arise from the same cause, for if the body was poorly nourished such active organs as the heart and brain would also show signs of impaired nutrition. The view that these cases were really intestinal in origin was arrived at because I found the whole train of symptoms present in cases with demonstrable disease, of some portion of the intestinal tract, with subsequent relief after an operation; as, for instance, in gastric ulcer with pyloric stenosis. The

removal of a diseased appendix has also been followed by a recovery.

The following cases illustrate experiences, of which I have had a great many, somewhat similar. I saw a woman, aged 52, who for the previous five years had led an invalid life on account of supposed heart trouble, as diagnosed by a distinguished physician. She could take but little exercise on account of exhaustion easily produced. She had at times a good deal of aching in her left breast and arm, particularly when fatigued. Her digestion was fair, and she made no particular complaint, though she admitted she had to be careful with her food, and that she had constipation and pain in the abdomen in her youth. She was pale but did not look ill. There was a good deal of hyperalgesia of the left breast, and the heart was slightly enlarged. On examining the abdomen, the skin over the lower half, on the right side, was found distinctly tender on pressure, and on lightly pinching between the thumb and forefinger. She stated she had been aware of this pain and tenderness at various times since she was 23 years of age.

Another woman, aged 38, consulted me because of attacks of angina pectoris, at times of great severity. She had consulted several experienced physicians, and one, an author of a book on diseases of the heart, had warned her husband that the attacks were genuine angina pectoris and the outlook was grave. She had the hyperalgesic areas in the left breast and abdomen as described in the last case. There was, in addition, rigidity and tenderness of the muscles over the right iliac region. I suggested an operation, and an old inflamed appendix with numerous adhesions was found. The removal of the appendix and breaking down of the adhesion was followed by a cessation of the attacks of angina pectoris, and when I last heard of her, four years after, she was in good health.

The following case is a typical illustration of a number of cases I have seen, and as it throws a good deal of light upon the attitude towards medicine of many physicians and surgeons, and suggests a probable cause for the predisposition towards consumption, I give it at some length. I had known the family since the patient's childhood. He had no illness till about the age of 30, when he consulted me for being slack and depressed, and easily exhausted, with occasional attacks of discomfort in the abdomen. He was thin and was losing flesh slightly. I could detect nothing wrong, except a somewhat diffuse tenderness of the skin and muscles on the right side of the abdomen, and a splashing of the stomach below the umbilicus. I gave some general directions as to his food and general health, and for a time he improved. He relapsed however, and I took him to see a distinguished surgeon, who poked his finger in the belly in the region of McBurney's point, and eliciting pain, declared that it was a case of appendicitis. I was doubtful, but in view of the surgeon's great experience, I consented to an operation. A long innocent-looking appendix was removed. After recovery from the operation there was no improvement. A year after he was seen by a physician skilled in mental affections, who declared the condition to be one of melancholia, and recommended a course of treatment by suggestion. His condition continued to vary in spite of various treatments, and I was still convinced that there was some intestinal trouble at the bottom, and he was next seen by a physician who specialised in intestinal diseases. After a test meal and a bismuth meal and X-ray examination, he pronounced the intestinal tract free from any stasis or disease, and diagnosed the case as one of neurasthenia. I did not see him again, but I heard of and from him from time to time. He next (at the age of 38) consulted another physician.

who diagnosed some form of dyspepsia, and took him into a nursing home for six weeks, where he was carefully dieted. A slight improvement followed, but a year later I had a letter from him from a surgical home, telling me that a few days before he had been operated upon, and a duodenal ulcer was found and a gastro-enterostomy had been performed. From this he recovered. Shortly after he became troubled with a cough, and tubercle bacilli was found in his sputum. He was a well-to-do man and lived in the best hygienic circumstances, and the best advice was sought and followed for the tubercle infection, but he drifted, and died of consumption at the age of 42.

Arbuthnot Lane's Views.

I had reached thus far in my inquiry into this condition, and had collected a large number of cases and carefully studied their symptoms. In my practice digestive troubles were very common, and I was never without ten or a dozen cases of gastric ulcer under observation. The question of why people should have gastric ulcer was continually before me, and I could get no answer to this, nor find an intelligent reason for the ill-health of those who suffered from the X-disease, until I read Arbuthnot Lane's researches. When I became familiar with them, I saw they afforded a probable explanation of these states, and a hypothesis which seemed so reasonable as to afford a guide for further investigation. The basis of Lane's views is the result of a long-continued and profound study of the reaction of the tissues of the body to strain. These studies date back to his student days, when he began to examine the bony skeleton in men exposed to different forms of severe bodily labour. This line of study was continued by an inquiry into the changes that take place in fractured bones when not accurately set, show-

ing that stress may cause absorption of the old bone while a new shaft is developed. He also studied the formation of new joints to meet extra strains. Passing from bony changes he studied the effects of strain on soft tissues, as the ligaments of joints, and was led on to consider the manner in which the various portions of the digestive tube are maintained in position. The evolution of man in the course of his life from a crawling child to an erect man, must obviously involve an alteration in the stresses put upon certain organs. Naturally, the support of the weight of the intestines must alter, and if the mesenteries are ligaments of support, they must of necessity be strengthened to prevent the bowel falling into the pelvis.

But other influences occur. The human being in its early days defecates when nature calls. Civilisation, as Leonard Williams puts it, requires that the child should become constipated for twenty-four hours every day. This necessitates a retention of material in the lower portion of the gut. The weight of this must be supported, and so there results a thickening of the mesentery at the sigmoid flexure to support the weight. As time goes on the accumulation of material extends further up the tube, and similar strengthening supports are developed at the splenic flexure, at the cœcum and at the duodenum.

All recently evolved structures are unequal in their development, tending to excess or insufficiency. In time, many of these supporting bands cause a sagging of the loaded bowels, which interferes with the passage of the contents. These accumulate and decompose, affording breeding grounds for bacteria, noxious and innocent. The body becomes infected by the bacteria, or the health becomes impaired from the absorption of the products of decomposition. At certain portions of the digestive tube, the stasis interferes with the normal

mechanism, as at the pylorus, where the congestion and friction impair the nutrition and lead to the formation of ulcers in this neighbourhood. The appendix and the ovaries become involved in the adhesions that are formed in the development of the supports, and this leads to diseases of these organs.

The impaired health, from the bacterial invasion of the body, or by the toxins, lowers the resistant powers, so that pathogenic organisms gain a foothold and produce secondary diseases. Lane gives a list of diseases which he considers secondary to this condition of intestinal stasis, many of which he claims to have cured by operative procedures which have removed the stasis.

When I grasped the full meaning of Lane's conception, I began a more systematic examination of my patients, to see if I could get any direct evidence, and I got enough to satisfy me that the subject was well worth careful investigation. For many years I had noticed that many patients, with abdominal troubles, had areas of skin which were frequently tender on pressure, and that under these areas the muscles were often more or less in a state of tonic contraction. I had recognised that these signs invariably indicated some diseased condition of the organs in the abdominal cavity, and were often associated with peritoneal adhesions. In watching these cases a certain number came to operation, as in the second case cited above, while others developed symptoms of appendicitis, and at the operation adhesions such as Lane describes were always present.

My opportunities until recently did not permit me to carry on a research in this matter. Recently I asked my friend, Dr. Waterston, Professor of Anatomy in St. Andrews, if he would look inside some abdomens, of the subjects that came to the dissecting room, for Lane's bands. He readily agreed. The first one, an elderly

subject, that was opened, might have been the individual from whom Lane drew his description. There at the sigmoid flexure was a strong band binding down the bowel and narrowing its lumen. There were a series of adhesions at the splenic flexure. The cœcum was fixed by a mass of adhesions in which the appendix was embedded. There was a dense series of adhesions fixing the duodenum to the under surface of the liver and diaphragm. Another body opened showed these bands and adhesions in a much less degree. A newborn child was opened and examined, and no sign of anything corresponding to the adhesions and bands at the sigmoid flexure, or the cœcum, could be found. An extended investigation by Professor Waterston reveals that adhesions, especially around the cœcum, are so extensive that other factors than stress must take part in their production.

When I found that Lane stated that the lowered health, resulting from intestinal stasis, rendered the individual susceptible to the tubercle bacillus, I cast my mind back on the cases of consumption that I had seen in my practice. I found, on reflection, that among these, my regular patients, a number had contracted the disease while under observation. The majority had been in poor health for months or years before the disease was suspected, and I had attended at least half-a-dozen for gastric ulcer, like the man whose case I have quoted. When one reflects on the complete failure which has resulted from the many attempts to understand the true nature of consumption and its remedy, a view so reasonable as Lane's merits a very thorough investigation. The fact that the great majority of persons suffer from a tubercular infection, but only a few—and these living often under the best hygienic conditions—develop consumption, suggests that there is some other factor at work than the tubercle bacillus.

The failure of all modes of treatment, in cases where the disease has got a grip of the patient, suggests that there is some factor underlying the condition, whose removal is necessary as a preliminary step to cure.

I do not wish it to be understood that I consider Lane's views are proved correct, but I mention them in order to call attention to a phase of medicine that has not received due consideration, and in order to show that such a hypothesis as this serves as a guide for anyone who wants to further medical progress, even if the search is undertaken to disprove it. By so doing he will bring to light new facts which will help to a better understanding of the subject.

CHAPTER IX

THE PLACE OF THE PHYSICIAN

The need for teachers with a wide outlook.

In medical schools there ought to be a teacher, or several teachers, whose experiences enable them to take a wide outlook on the whole field of disease. The breaking up of medicine into sections now renders it impossible for such a man to have a place in a school. The result is that the students never see the different sections in their proper perspective. The man who should take this position is the physician, and he ought to be the dominant figure in medicine. It is he who should see disease in all its phases. The problems to be solved are continually presenting themselves to him, and it is he who should recognise the problem and direct the laboratory workers and others to its solution; and it is he who should specify to the surgeon the time when operative proceedings should be undertaken. But in order to attain this position, he must make himself familiar with all the manifestations of disease, so as to recognise the conditions that provoke the disease, the first sign of its invasion of the human body, and its varying manifestations as it runs its course. He should recognise whether the disease is amenable to treatment, and should seek for the appropriate treatment.

At one time the physician did occupy a dominant position, but during the last 50 years the laboratory workers, and the surgeons, have made such progress in their special departments that they usurp the physician's position. To a great extent the physician is

himself to blame. He has allowed himself to be so dazzled by the inventions of his colleagues, and has accepted their methods and ideas, not recognising the limited scope of all such methods, while his own vastly more important field has been neglected. To such extent has this abnegation of the physician taken place, that the main road to a physician's post often lies through the laboratory, where he is trained in such a limited field that he never obtains that wide outlook which is essential, especially when he has to teach students who, by force of circumstances, will see disease in a far wider aspect than any of the teachers have experienced. The responsibility on the physician is very great, for it is to him that the student looks for placing the essentials of his training in the different branches in their due perspective in their application in practice. This at present he is supposed to do without having seen what the general practitioner's life is like, so that he has no knowledge of his students' opportunities for seeing disease, and does not know the problems that confront the doctor in the practice of his profession.

Principles which should guide medical education.

If it be admitted that success in the prevention and cure of disease is more likely to be achieved after a knowledge of the manifestations of disease, and the conditions favouring its onset are fully understood, it follows that the investigator must have the opportunity of seeing the affected individuals at all stages.

If we look over the whole field of medicine and contemplate the different workers to recognise which worker has the opportunity for finding out the facts, essential to an intelligent prosecution of research, we can find only one type of worker who fulfils the requirements, and that is the general practitioner. Some idea of his opportunities and of how these can be utilised, I have

attempted to give in the recital of my personal experiences.

To educate the general practitioner so that he should make the full use of his opportunities should be the aim of his teachers. This requires that his teachers should not only recognise the opportunities the general practitioner has, but they should be able to understand on what lines his education should be pursued, to enable him to make the best of his unique opportunities.

In order to accomplish this object a teacher must himself recognise from experience what subjects should be taught, and he should also be able to recognise the subjects, at present taught, which should be left out of the student's curriculum.

Essentials in the training of the Physician.

Unfortunately these requirements in a teacher have never been recognised. So far, indeed, are they from being recognised that steps are actually taken to prevent him acquiring the necessary knowledge.

There has arisen a tradition that only a select body of men are competent to fill a teaching post. This is the result of human frailty. If, in any sphere of life, a few individuals are set apart and given power over their fellows, it is but human that such a select body will seek to aggrandise their position at the expense of their fellows. It is unnecessary to enlarge on this general statement, as the world's history shows its truth in all spheres of human thought and action. In some spheres it may be good, or it may be bad, but in intellectual matters, and especially in science, it can only be bad. In medicine, the teachers have practically taken into their own hands the guidance of education, and all the intellectual interests of the profession, and ordain the course a man must follow who wants to become a doctor.

We have in London a body—the College of Physicians —which has acquired the power indirectly of preventing anyone who is not a member of their body obtaining an appointment as a physician on the staff of a teaching hospital. A young aspirant to such a post must follow certain lines which custom prescribes. He can exclude himself from having any personal contact with patients, by spending his time in a laboratory, undertaking what is called "research," and this is the surest way of attaining his object. He may spend his time in any other form of academic life, but one method he must not pursue—he must not attempt to qualify himself efficiently for such a post by the experience of general practice. If, for instance, he wishes to see the kind of life his future students would lead, or if he wishes to investigate the early stages of disease, and for that purpose undertakes general practice, he will by such a step render himself unfit for membership of the College of Physicians, and so cut himself off from any chance of obtaining a position as a physician to a teaching hospital. The College of Physicians practically requires of their members that, while they may pursue almost any one of the branches into which medicine is split up, they must not practise medicine in the only way by which a wide outlook may be obtained, and so render themselves fit and capable to become really effective teachers. With such a glaring instance of the present day methods it is not surprising to find that there is not an individual on a teaching staff, qualified from experience, to see all the branches of medicine in their proper perspective.

I am aware that the argument is that the College of Physicians must maintain a high standard of personal conduct in a profession exposed to ways that tend to degradation. But this limited outlook tends to foster inefficiency. It is a matter of common knowledge that

to become a consulting physician the membership and fellowship of the College is a necessity. The surest way to get these qualifications is to avoid general practice. One special function of a consultant is to foretell what is going to happen to a patient if treated or if left untreated. To obtain this knowledge it is necessary to see patients through the various stages of disease, and this can only be done by the individual who has the opportunity. The College of Physicians thus prevents consultants following the only way in which this knowledge can be obtained. I have known laboratory-trained young consultants actually refuse to see patients regularly, lest they should be considered general practitioners. When I have pointed out to them that they can never assess the value of symptoms unless they watch individual cases of disease for long periods, I have found them incapable of recognising the need for such a knowledge!

The Training of the Physician.

If it should be recognised that it is an imperative necessity that among the teachers of a school of medicine there should at least be one who has a broad outlook on medicine, and is therefore qualified to place before the student the position of the different branches in their due relation to the practice of his profession, the physician is the individual most likely to be possessed of this kind of knowledge. Once the necessity for this kind of teacher is recognised, the insufficiency of the present methods of educating him will be appreciated.

It would be of little use at the present time putting forward any suggestion as to the reorganisation of medical education. While there is a widespread recognition that medical education is defective, there is no clear conception of where the defects lie, and until this

is fully realised, attempts at improvement will fall far short of the requirements. Much of the instruction to-day deals with unorganised details, and the need for the search for principles is scarcely realised. In course of time these will be realised and teaching will become greatly simplified.

In the meantime there should be in every school of medicine one or more teachers who have been in general practice for 10 to 20 years. Indeed, it would be well if a young man were encouraged to enter general practice with the expectation that if he proved himself fit he should be qualified to become a teacher, and have the opportunity in a well equipped hospital of solving the problems he has had to face in his practice. In general practice he would be speedily brought face to face with the real difficulties of medicine, and being thrown upon his own resources, he would soon recognise from experience the problems to be solved. He would see every phase of the more common diseases, and have to practice every branch of medicine. In saying that he would have to practice branches of medicine which are generally considered so important that it is necessary for men to spend their whole time at that special work, I am but stating what actually happens in practice, and the majority of patients have to be content with the general practitioner's knowledge. And after all, it is not very difficult for the general practitioner to become fairly efficient in these special branches. Many general practitioners are very efficient obstetricians and surgeons, and it is easy to acquire a very practical knowledge of eye, ear and throat diseases. It must be borne in mind, that seeing great numbers of patients suffering from a particular disease is not the means by which a knowledge of that disease is acquired. The general practitioner who studies carefully such a disease as appendicitis, noting its symptoms from its

onset, the condition at operation, and the after results in a dozen cases, will gain a far better knowledge of the disease than the surgeon who has a thousand operations to his credit, following the practice of to-day.

An experience such as this, carried out by a man who has recognised clearly the aims of medicine, and has realised the defects in the knowledge of medicine, is bound to give him an outlook absolutely essential for a teacher. If his experience has been one of 10 to 20 years, he would have had the opportunity for knowing how to assess the value of symptoms, which is so essential for a consultant and a teacher to know. One can readily see how a teacher, trained in this way, can influence his students, and can tell from his experience what branches, and what of each branch of medicine, is important for the student to know. By such experience only can the whole field of medicine be viewed in its proper perspective.

If it was recognised that the posts of physicians to teaching hospitals, and consulting practice, were open to men who would train themselves in the field of general practice, a greater incentive would be given to general practitioners to utilise their opportunities. The results of their observations being published, it would not be difficult to select men of capacity. No teacher of clinical medicine should be appointed until he had demonstrated that he had abundance of experience, and the ability to advance medicine, so that a qualification for a teacher would be from ten to twenty years' practice of his profession, in actual contact with patients. This length of time is absolutely necessary for a man to acquire, not only the power of detecting the manifestations of disease, but of assessing their value, for it is evident that no one can attain this latter power unless he has himself followed individual cases a sufficient length of time.

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